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LECTURES ON SURGERY, MEDICAL AND OPERATIVE,

DELIVERED AT

St. Bartholomew's Hospital;

BY MR. LAWRENCE.

LECTURE LXXXVII.

Operations on the subject continued.—Amputations of the Thigh, Leg, at Joints of the Fingers, Wrist, Hip, and Shoulder.

I PROPOSE to show you this evening the mode of performing some of the more ordinary amputations. Now there are a variety of points for consideration, if we speak of amputation generally. In the first place, we should have to consider the circumstances under which amputation is necessary or expedient; but as in the former parts of the course, when speaking of gun-shot wounds, compound fractures, and diseases of the joints, I have had occasion to state to you under what circumstances amputation is required, I have now merely to show you the mode in which the operation is to be accomplished.

There are two methods generally adopted for the removal of limbs—amputation by the *circular incision*, and the *flap* operation. In the former of these, a circular cut is made through the soft parts, and a subsequent division of the bone, making the wound of the soft parts such, that the edges can be neatly approximated, and placed under circumstances favourable to the union by adhesion. In the other case, the soft parts are divided by one, two, or even a greater number of incisions, not in a circular manner, but so that when the limb has been removed, they admit of being adjusted and fitted to each other, and of course of being placed also in the position most favourable to the union by adhesion. Of these two methods, the first is most frequently employed.

We have now to consider the situation in which amputation is to be performed, as well as the mode of performing it; and, generally speaking, the rule of proceeding is to amputate the limb so as to preserve as much of it as we can, without leaving any of the disease that requires the operation. The situation in which amputation is to be performed, differs under different circumstances. Amputation may be performed either in some portion of the continuity of a limb, or at a joint; we may cut off the limb either between two joints, or by making the division at one of the articulations. The former mode is the one most commonly adopted. Then, in reference to the performance of the operation on any particular occasion, we have to consider the proceedings that are necessary in order to arrest the hæmorrhage, the mode of performing it in the quickest and most prompt manner, so far as the operation itself is concerned,—that is, the immediate mechanical removal of the part—the steps which are subsequently necessary for preventing future hæmorrhage, by securing the orifices of the divided vessels, the mode of uniting the wound, and the treatment of the patient after the operation has been performed.

Amputation of the Thigh.—I shall, in the first place, show you the mode of performing amputation by the circular incision, the proceeding generally adopted in amputation of the thigh. In this case we always adhere to the rule I have mentioned to you, of taking off the diseased or injured part in such a way as to preserve as much as possible of the sound portion of the limb, making the incision as near to the knee-joint as we can. It is necessary, of course, that the whole of the disease should be removed; we should, therefore, perform the incision completely above the part to which it may have extended; but in many cases there is some kind of exception to this rule: suppose we amputate for disease existing in the knee-joint; in this case abscess may have extended to a great distance along the thigh; it may have reached perhaps from the joint half way up it, or even further. Now it is not necessary on that account to

make the incision completely above the situation to which such abscess may have reached. In these cases, the mere formation of matter in a part of the limb is no objection to the incision being made through it; for inasmuch as you take away the primary disease that has led to this formation of matter, you will find that the change of structure consequent on such abscess, does not at all interfere with the healing of the wound. You may cut through a sinus, or, occasionally, even an abscess, without at all endangering the success of the operation. In a case, therefore, where the sinus or the abscess extends considerably above the joint which requires amputation, you do not require to go so high with your incision, as to remove the whole of the diseased parts. But if you were removing a limb in consequence of fungus hæmatodes, melanosis, or any cancerous or funguid disease, you would be very cautious to go completely above it; not even to amputate in a sound part immediately above, but at some distance above the situation to which it may have reached. In the case, therefore, of amputation in the thigh, on account of disease of the knee-joint, or of some other affection of the limb, we endeavour to amputate near to the knee—to preserve the thigh as long as possible, by means of which we very much facilitate locomotion afterwards.

The first point in amputation, is to apply the tourniquet so as to stop the circulation through the main artery and prevent bleeding during the removal of the limb. Bleeding in an operation of this kind may be prevented by tying any circular bandage round the limb with a certain degree of force. It is not absolutely necessary you should employ the instrument called a *tourniquet*. If you merely place an ordinary ligature round the limb and draw it very tight, that will be sufficient to prevent hæmorrhage. The surgical instrument, however, called a tourniquet affords a more safe and convenient mode of commanding the bleeding. In this instrument you have a strong girth which buckles round the limb, a part which is called the pad which slides along the girth so as to admit of its being pushed into different situations according to the size of the limb, and a screw by means of which, after you have buckled on the strong band, the pressure can be increased to any extent you please; this is the construction of the tourniquet. The object in amputation is to divide the soft parts so that they may be conveniently brought together again after the operation has been performed, and may completely cover the extremity of the divided bone, inasmuch as the soft parts will contract considerably after division, therefore it is necessary to saw through the bone higher up or nearer the trunk than where you have divided the soft parts. For this reason it is expedient to give a free power of retraction to the muscles after you have divided them; and you apply the tourniquet as far as convenient above where you are going to make the incision, so that the muscles when cut through shall have full scope to retract. The pad of the tourniquet is to be placed immediately over the artery, so that when the screw is turned, there may be a sure pressure exercised on the vessel. It is expedient to have it as near as you can opposite to the screw. You will find it expedient to draw the tourniquet very tightly before you begin to screw it, or otherwise, particularly if the limb is thick, you will find you will have to turn nearly the whole length of the screw in order to give the necessary degree of pressure. Having applied the instrument in this way [*The lecturer showed the different steps as he proceeded*], you turn the handle till you have got it pressed as firmly as you consider to be necessary in order to prevent the passage of the blood through the artery. Now it is of course necessary that the operator before he makes the first incision should satisfy himself respecting the position of the tourniquet, the place on which the pad presses, and the sufficiency of the pressure for arresting the flow of blood. The next object in proceeding to amputate by the circular incision, is to divide the integuments and the adipose membrane by a single cut; having done that, you retract the divided integument, detach it to a certain extent from the subjacent muscles, and divide them by a second cut down to the bone; which you then proceed to saw through. Thus there are three points in the operation, the division of the integuments and adipose membrane, the division of the muscles and other soft parts, and the division of the bone. The integuments when divided must be retracted by the hands of the assistant; you should detach the adipose membrane which connects them with the muscles so as to have a sufficiency completely to cover the muscles and the bone; and to admit of their being brought together in a straight line. It is not sufficient merely to divide the skin and adipose membrane; it is usually necessary to turn back a certain portion of the skin so that the muscles may be divided higher up than the integuments were. This becomes the more necessary in proportion as the limb is thicker or larger at the part where you operate, and also in proportion as the muscles and other soft parts may be more consolidated,—rendered more firm by the inflammation they have undergone,—for in that case they will not retract very considerably, nor give way when you come to press the sides of the wound together, and consequently the whole

surface which you have made, when you come to divide the bone, must be covered by the integuments you have preserved. You should calculate the length to which the integuments should be left in order to cover the muscles and the stump, if you suppose that the parts will be unyielding. You might measure the circumference of the limb; say that this is twelve inches at the point where you make the incision, the diameter at this part will be four inches, and when you come to approximate the integuments you must have on each side one half of the extent of that diameter; that is, you must have turned the integuments back two inches in the dissection, supposing you depend entirely on the dissecting back of the integuments in order to cover the face of the stump. It is not absolutely necessary to divide the adipose membrane and integuments at one incision, but you can usually accomplish the purpose in that way, and it appears the most dexterous mode of doing it; all you have to do is to carry your hand round the thigh, bring it back, and cut through the integuments by one stroke; if you do not find that very easy, you may begin on the inside of the limb, carry it round, and cut the parts in that way. I begin then by making the incision very close to the knee-joint; for you may recollect, that although I cut very close upon the upper edge of the patella, I shall not have to divide the bone in that situation, but perhaps three or four inches above it. This is the first step of the operation. I shall now just dissect back the edge of the integuments, and I may observe to you that it is not necessary to be very accurate in making the first incision so as to divide the integuments only; if you cut a little of the muscles, it is not very material, while on the other hand it is necessary to make the incision completely through the integuments and adipose membrane. I have now cut through the integuments, and turned back a little all round. The second step of the operation consists in cutting at the point to which you have reflected the integuments circularly through the whole of the remaining soft parts down to the bone, and in doing this you must be careful not to cut into the integuments, otherwise you may make a button-hole in them, which, although not very material, is not very neat; the knife must be carried close upon the part to which you have reflected back the skin.

Now when the muscles are thus divided, you find that they retract unequally; some of them are loose, not adherent to the bone, and others are fixed closely to it; and the former retract much more than the latter do. After you have divided both then, you will draw up the parts with your hand in this way, and then carry down the knife to the bone again, cutting through the thickest muscles higher up; and in the living subject, where it is desirable to saw through the bone as high as possible, you will find it necessary perhaps, to carry the point of the knife a little inwards down close upon the bone, so as to detach the muscles where they adhere immediately to it. Your object will be in general to saw through the bone as high up as you can, in order to preserve a proper quantity of the soft parts to form the stump. Now, the mode which I have shown is called amputation by the *double incision*; for heretofore it was the practice of surgeons to cut through skin and muscles down to the bone at once; a mode of proceeding which would necessarily be attended with this inconvenience, that the skin would not be sufficient to cover the surface of the muscles, which would thus project uncovered at the end of the stump, and produce a large circular wound equal in size to the diameter of the limb. It was soon found, therefore, to be a much more convenient plan to cut through the integuments, to turn them back, and then to cut through the muscles in such a way as to leave them capable of being brought together and covered over, than to leave the stump with the wound I have alluded to: then having carried the operation to this point, it only remains to saw through the bone, dividing it without injuring any of the soft parts; for which purpose, it is most convenient to employ a *retractor*, a piece of stout linen divided at one end into two strips, which are passed on either side of the bone, and drawn close together above it, so that the assistant by means of the retraction can hold the soft parts out of the way while the bone is divided with the saw. The retractor is not absolutely necessary, but it is more safe and convenient to employ it, as by it you avoid all risk of injuring the soft parts.

Now you will observe, that when a limb has been removed in this way, the part removed represents a cone, the basis of which is formed by the integuments and the apex by the bone; you have divided the integuments therefore considerably lower down than the muscle, and the muscles lower down than the bone. Consequently, you find that the wound thus formed, admits of being united so as to represent a straight line across the face of the stump, either transversely or in the opposite direction. The next point is to secure the divided vessels; and in the first place, knowing the situation of the main artery of the limb, you take hold of it with a pair of forceps and draw it out, separating it, if it does not immediately come away, with the forceps from the parts to which it is connected, and then putting a ligature on it; there may be on

or two other arteries to be treated in the same way; and having secured them, you will generally find it expedient to loosen the tourniquet in order to see what other vessels bleed. If you find two or three arteries bleeding very freely, you may loosen the tourniquet partially, and secure them; but after having secured two or three, you generally find that it may be taken away entirely, for as long as it is employed even partially, there is a great discharge of venous blood; which, after it is removed entirely, is put a stop to altogether, and then you can better make out what are the arterial orifices that produced the hemorrhage. The number of vessels that require ligature after amputation, is very variable; sometimes the femoral artery and one or two others, are all that require to be tied; in other instances, you may have ten or even fifteen to tie, after amputation of the thigh. It is expedient to secure by ligature all the bleeding vessels before you dress the stump. It is a very perplexing circumstance to have hemorrhage occur after the operation; to be called to the patient in the evening or the night after the operation, and to be obliged to undo the dressing of the stump in consequence of the bleeding. It is very painful to the patient, and very inconvenient to the surgeon; it is much better to occupy a little more time in securing the bleeding vessels at once, than to subject ourselves to an inconvenience of this kind. After securing the femoral artery, you often find that hemorrhage continues from the femoral vein, and that a pretty considerable stream will issue from it. It is very desirable to avoid applying a ligature to a vein, for the application of it frequently produces inflammation in the venous trunk of the most dangerous kind, often terminating fatally: it is better therefore for the surgeon to press with his finger on the orifice of the vein for a short time, and proceed first to secure any other bleeding vessels, and perhaps by the time he has secured all the others, the bleeding from the femoral vein may have ceased: or he may apply a temporary ligature tied merely with a single knot, taking it off when he has tied all the other vessels; when, if there is no further bleeding, it is not necessary to apply it again. If, however, at the end of the operation, the stream of blood from the vein is still considerable, it is of course, at last, necessary to apply a ligature to it. It has happened to me in a great number of instances, to tie the femoral vein; I do not however recollect that any inconvenience arose from it. If there is bleeding from small vessels, you can often check that by sponging the stump with cold water, a proceeding which it is as well to employ in every case of amputation, as it assists the contraction of the orifices of the vessels, and thus tends to diminish the number of those that may require to be tied.

Having secured all the bleeding vessels, the next point is to dress the wound; and in doing this, you follow the general rules that I have already laid down for the approximation of recent wounds; that is, you bring the divided parts together, and retain them in apposition by strips of sticking-plaster, and you trust to this mode, to induce union by adhesion of the two sides of the wound. You may unite the wound in a line across the surface of the stump, or in the opposite direction, according as the soft parts can be most conveniently approximated. I do not know that either way particularly deserves the preference; but perhaps, on the whole, the method in which the sides of the wound are brought together laterally, so that it forms a straight line from behind forwards, is the most desirable. You employ, first, a broad strip of sticking-plaster across the middle of the wound, passing it up nearly the whole length of the thigh on both sides, and bringing the edges properly together, not making a point of drawing them forcibly, or very accurately, into apposition, and then as many more strips as will keep the sides in contact. It may be as well to leave the lower angle of the wound a little open, for the escape of the blood which sometimes oozes out; for when this has no escape, it may excite irritation in the wound, and so induce further hæmorrhage. The ligatures that have been applied are to be brought out as nearly as possible opposite to the situation of the vessels on which they have been applied; they may be brought out at the lower angle of the wound, as that is the situation in which, if there is any bleeding, the blood will the most readily escape. No further dressing is necessary than what I have mentioned; at all events, nothing more is necessary than a portion of lint spread with cerate, and laid over the strips in order to prevent them from being entangled, and this may be secured by two small strips of plaster over it. It has, however, very commonly been the practice to apply a circular bandage, beginning from the bend of the thigh, and carrying it downwards; to apply a large portion of lint over the wound, and to secure it by bandages and portions of plaster placed across the end of the stump, and, further, to include the whole of the parts after this dressing in a bag or woollen night-cap, drawn over the stump. This is the mode of dressing that was generally employed in this hospital some years ago. It is, as you will observe, contrary to the general principles that ought to be followed in the management of recent wounds. It is covering the wound with substances calculated to hear it, and, of course, to favour

the occurrence of inflammation, and thus to frustrate the objects we have in view—union by adhesion. In this case, as in that of any other recent wound, we must endeavour to keep down inflammation—we must do all we can to prevent the increased vascular action of the part going beyond the extent sufficient for the agglutination of the sides of the wound; therefore, the cooler we can keep the wound, the sides of it being simply held together, the more likely are we to have a favourable result; while the more we cover it, the more likely are we to excite heat in it, and to have inflammation going on to suppuration. There are some instances in which the muscles of the limb are very loose and flabby, where the patient is much emaciated and enfeebled, where there is little chance of inflammatory action taking place, and where we deem it expedient, in order to keep the parts together, to employ a circular bandage from the bend of the thigh down to the end of the stump. It must be applied very tightly; and I think the surgeon who has performed the operation, when this bandage is applied, ought to take care to see the patient a few hours after, or depute some competent person to do so, with instructions to divide the bandage, if it shall have been tightened by the swelling of the parts. At the subsequent dressings, after the operation, nothing further is to be done than what comes under the general principles which I have had occasion to lay down respecting the management of recent wounds.

Amputation of the Leg.—In the case where there are two bones to be divided, the mode of proceeding is essentially the same as where there is only one. The leg offers the most common example of this species of operation. The principal difference in the case is in the shape of the retractor, which is divided into three portions instead of two; for a piece of it must pass up in the interval between the two bones, when you come to saw through them.

Now, in amputating the leg, we do not observe that rule which I have said is applicable to amputation of the thigh—we do not attempt to preserve as much of the limb as possible. Supposing a disease of the ancle-joint to be the case requiring amputation, and that it should be limited to the ancle, we do not operate just above the ancle, but at a certain distance below the knee. It is found that, in reference to the subsequent wearing of an artificial limb, the body may be much better supported by applying the artificial limb to the natural surface of the knee than to the extremity of the stump, especially when it is amputated low down; the rest upon the bent knee is by far the most convenient to the patient; and although, in the other way, there may be a much greater length of the extremity, it does not, in general, answer so well, as the small end of the stump is apt to ulcerate, and does not, at any rate, accommodate itself to the artificial limb, so well as the end of the knee does. We operate therefore on the leg in such a way as to divide the tibia and fibula at about four fingers' breadth from the articular extremity of the tibia, and that leaves a pretty good surface for the limb to rest upon, when it is attached to the artificial leg. Amputation of the limb lower down has been frequently tried, from the wish to save a larger portion of it, but has not been found to answer so well.

I consider, too, that the circular incision answers best in the case of the leg, although this is a part on which the flap operation has been repeatedly practised, and for which it was, in fact, originally proposed. In this operation, the flap is cut from the calf of the leg; either a sort of oval incision is made through the integuments, and then the muscles are divided, or a sharp-pointed instrument is carried through the calf, and the flap is made from within outwards; for which purpose this instrument called a *catling*, a double-edged knife with a sharp point, is well adapted. I will just show you this mode of proceeding.

Carry the instrument, in the first place, through the muscles of the calf, close to the posterior surface of the tibia and fibula, and bring it out obliquely backwards and downwards; in this way you get a flap of the soft parts, which will apply to the surface of the stump when you have finished the operation. It is necessary in amputating the leg, to reflect the muscles a certain distance along the posterior surface of the tibia, because there are there no other soft parts in front to assist in forming the stump. Then whether you operate by the circular incision or by the flap operation, you must divide the muscles that lie just between the bones; you do this whether you are operating on the leg or fore-arm, with the catling, passing it between the bones, and dividing first the muscles upon one, and then those upon the other. I pass it through, as you observe, from before, and carry it down close upon the tibia, so as to divide all the muscles; I carry it in the same way down upon the fibula, then pass it between them so as to divide the muscles in that direction. It is necessary here, on account of the close adhesion of some of the muscular and tendinous fibres to the bones, to be particularly careful to divide the whole of the soft parts before you begin to use the saw. Then in applying the retractor, the middle of these three divisions, as you see, is carried in the interval between the bones. In divid-

ing the tibia and fibula, the surgeon usually places himself on the inside of the limb, so that he can divide the bones together. This is not absolutely necessary; you may saw through the fibula first, and the tibia afterwards, or *vice versa*; it is not absolutely necessary that you should divide the two bones at once. If a small portion of the muscular substance should be observed to remain adherent to the bone, just divide it with the knife, and do not allow it to be torn through by the saw. Then you bring the edges of the wound together by drawing forwards the flap that has been formed of the muscles of the calf; you observe that you may bring them together very evenly after the soft parts have been divided in this manner; but for my own part, I consider that the circular incision is the best for the removal of the limb in the situation in which amputation of the leg is usually performed, and the subsequent union of the wound by the approximation of its sides laterally, just in the same way I have recommended you to unite the sides of the wound in amputation of the thigh.

Amputations at Joints.—There are certain situations in which we amputate at the joint by preference, and there are other circumstances under which we amputate at the joint by necessity. To the latter cases belong amputation at the shoulder and the hip-joints; to the former, amputation of the fingers and at the wrist.

Amputation of the Fingers is a very simple affair indeed; it is performed by a circular incision, carried a little beyond the articular extremity of the bone which you wish to amputate. You make a circular cut, leaving just as much integument as will be sufficient afterwards to cover the extremity of the bone, then draw the integument back; and usually in the living subject it retracts sufficiently without dissecting it back; you then cut into the joint, either on the external or the internal side. The removal is, as you see, accomplished with great ease, and the integument thus left forms a sufficient flap; and in this way you perform amputation at any of the joints of the fingers.

It is sometimes advised, that the cartilage covering the articular extremity should be removed when you amputate at an articulation: this is not necessary; if you bring the parts together, they will unite very well over the cartilages; however, it is not difficult to pare off, or to scrape away with the knife, the cartilage from the end of the bone; there is no particular objection to it.

When you have to amputate at the articulation between the metacarpal bone and the first phalanx of the finger, you cannot make a circular incision through the integuments; the configuration of the part does not admit of it; you must, therefore, make an oblique incision—one on each side, and you will find it of advantage, in reference to the subsequent shape of the stump and the arrangement of the parts, to take away not merely the finger, but also the head of the metacarpal bone. If this is left, the metacarpus retains its natural breadth, and of course a very great gap is left between the fingers; whereas if you take it away, you will find that the two fingers come much nearer to each other—so near, that the gap will hardly be observed. In this way, then, you carry the incision from one side to the other, from before backwards, to the joint.

Now you may, perhaps, find it most convenient to separate the finger first, before you remove the head of the metacarpal bone, cutting into the joint the same as if you were removing the finger at the joint merely; for in this way you have the head of the metacarpal bone exposed; you may then carry the knife close to it on each side, so as to separate the soft parts; take it off at about three-fourths of an inch above the joint, by means of Mr. Liston's strong bone-nippers; the wound will then unite well—a very good stump will be formed.

Amputation at the Wrist-Joint.—You may amputate at the wrist-joint, either by a semi-circular incision, extending along the back of the carpus, and a corresponding one on the opposite side, or you may use the catling, and extend the incision from side to side, cutting from within outwards—it may be done either way. Carry the catling in along the back of the hand, and cut the flap in that manner; then cut into the joint at the back, and make the incisions in a corresponding way in front. In this way the hand may be speedily taken off. You may, if you please, cut from without inwards in either way; the skin comes together and makes a very good stump.

Amputation at the *hip* and at the *shoulder* joints is done, as I have mentioned to you, under circumstances of necessity, that of the fingers and wrist is performed by choice.

Amputation at the Hip-Joint.—With respect to the hip-joint, the necessity of amputating there very rarely occurs. There has been no amputation at the hip-joint at this hospital within the last thirty years or more. I have never seen it done or had occasion to perform it, and I apprehend the chances are, that none of the gentlemen in this theatre will ever be called upon to do it. It is hardly necessary, therefore, for me to enter much into the particular mode of doing the operation on this occasion. I once had occasion, in amputating the thigh, to cut so near to the trunk, that I took off

about two thirds of the trochanter major; and in that case I could, with the greatest facility, have taken out the head of the thigh-bone, and then it would have been an amputation at the hip-joint; and it appears to me in many cases, that the ordinary circular incision as high as you can make it, with the removal of the head of the bone, would do very well, and perhaps even better than the flaps which are usually made in amputating at the hip-joint. You may amputate by making these flaps, carrying in a long double-edged sharp-pointed knife near the external side of the artery, passing it through the back of the limb, and along the trochanter, so as to cut the flap on the outside, somewhere about the direction I now show you, and separating it up to the joint; you have the opportunity of tying the artery when you have carried the knife on the inside of it, as it is pretty much exposed; then you make the flap on the opposite side, and disarticulate the limb. I do not see, however, that this could be much preferable to amputating very high up by the circular incision, and taking out the head of the bone in that way after it has been exposed.

Amputation at the Shoulder-Joint.—Amputation of the shoulder-joint is rare in what is called *civil* practice, although it is not unfrequent in *military* service. I think it is said of Baron Larrey, surgeon in chief to Napoleon, that he has amputated at the shoulder-joint about 100 times. Now I do not believe that amputation at the shoulder-joint has been done twice here within the last thirty or forty years, therefore I conclude that it is not an operation very frequently to be performed. In amputation at the shoulder or hip-joint, you must trust to compression with the hand for arresting the hemorrhage; there is no opportunity of applying the tourniquet in these instances. In the case of the hip-joint, the artery must be compressed where it passes over the bone at the crural arch, and in the case of the shoulder-joint, where it lies on the surface of the first rib. It is expedient on this account, in both instances to perform the separation of the limb as quickly as possible; for although the artery in both situations may be effectually compressed—although you can stop the circulation of the blood through the limb by pressure, you cannot so completely depend on continued and effective pressure for a certain length of time, as to proceed at all slowly in the operation with safety. The movements of the patient, the fatigue of the person who makes the pressure, tend to render it important that you should be as expeditious as possible in the performance of the operation; of course you expose the patient to a considerable loss of blood when one of those large vessels is divided, unless effective pressure is made; it is necessary, therefore, that you should make your mind up as to the plan of dividing them and of performing the operation, so that you may go through the whole with great celerity; and, indeed, this is necessary in all operations; it is well to reflect—to carry the objects you have to accomplish in view—to make a plan in your own mind—to arrange the instruments, and so forth, so that you will have nothing to do after you once commence the operation, but go through a kind of mechanical process, the different steps of which you have already arranged. We cannot consider the dexterity of an operation is in proportion to the shortness of the time he occupies in going through an operation, but yet, *cæteris paribus*, we say that that operation is best performed which is done in the shortest time; of course it abridges the sufferings of the patient, which is an object of great importance.

Amputation at the Shoulder-joint is a flap operation; that is, you make a couple of flaps, which you cut out of the soft parts surrounding the articulation, and it is expedient to make the flap which contains the principal arteries, the last. These flaps may be formed either by cutting from without inwards, or in the opposite direction. You make an external flap cutting from the point of the acromion obliquely outwards and backwards to the edge of the axilla, and an internal one by cutting from the same point inwards. After making the external flap, which is to be held aside by an assistant, you cut into the joint, and separate the head of the bone from the glenoid cavity, before you proceed to divide the soft parts that constitute the internal flap; and having done that, you can grasp them in your hand, so as to command the artery, before you proceed to divide it. Now the surface of the joint is laid bare. Now I have got the head of the bone out of the socket, and the external or posterior flap is made; then carrying the limb forwards, I can grasp with my fingers this internal or anterior flap which contains the vessels, before I proceed to divide them; and this division I can accomplish, either by carrying the knife from within (carrying it, of course, round the shoulder) or from without. I now hold the parts in which the cavity is situated between my finger and thumb; of course I immediately look for the orifice of the artery, which there is no difficulty in seeing; then having tied it, I bring the part together, and thus you close the wound.

Lecture LXXXVIII.

Excision of the Head of the Humerus; Amputation at the Wrist—of the Forearm—Partial Amputation of the Hand or Foot.—Removal of Articular Extremities of Bones in Cases of diseased Joints.—Lithotomy.

The amputation of the arm at the shoulder-joint is generally required in cases of serious injury of the bone, such as those inflicted by gun-shot wounds, or in consequence of disease occupying the upper part of the bone, and extending into the joint. In some cases of this kind, the removal of the disease may be accomplished without taking away the whole limb; supposing the head of the bone should be broken and splintered by a bullet, and at the same time there should be a considerable wound of the soft parts, you may, by enlarging the wound, be enabled to remove the head of the bone, and the portion of it which is thus broken and splintered, without performing amputation, and leave the limb in a state in which, though its motions are considerably impeded, it may be very useful to the patient. You will recollect that when amputation is performed at the shoulder-joint, you cannot supply the individual with any artificial substitute for the limb that is lost; therefore, although the motions of the arm and forearm may be considerably less perfect than before the removal of the head of the humerus, they may still be of great use to the individual, particularly in the case of the right arm.

This operation of excision of the head of the humerus has been done in several instances, and generally with very satisfactory results, and has left the person in a much better situation than he would have been in, if amputation at the shoulder-joint had been performed. In any case, therefore, of injury or disease involving the upper part of the humerus, if you are satisfied that the disease is confined to the humerus, and that the scapula is uninjured, or that the disease does not extend to it, you may safely attempt this mode of relieving the patient. In other instances, where the condition of the member generally, forms the cause for the operation, of course the mere excision of the head of the bone will not answer the purpose; but where the disease is confined to the head of the humerus, or that part of the bone immediately adjoining it, this way of getting out the injured head may save the patient the more serious loss of the entire extremity. The operation may be done by enlarging the wound, so as to expose the head of the bone; or if there should be no wound, by making a sufficient incision, and cutting round the head of the bone, so as to enable you to dislocate it, bring it out through the opening you have made, and saw it off. No great difficulty has been experienced in the performance of this operation, in those instances where it has been practised.

Amputation of the Wrist.—I mentioned to you that amputation may be performed in the *wrist*-joint, so that where the disease or injury is confined to the hand, we may save the whole of the forearm. In amputations of the upper extremity, it is desirable to adhere to the general rule I have mentioned, of saving as much of the limb as possible; therefore, if you can perform the operation of amputation at the wrist, you will do so; if however the wrist-joint is diseased, you will of course operate above it, though as near the disease as possible. In the case of an injury by gun-shot, or otherwise, that requires amputation, you must, of course, perform the operation according to the situation of the injury.

Amputation of the Forearm.—If you amputate in the continuity of the forearm, the most convenient method is the circular incision, cutting through the integuments, and reflecting as much of them as will be sufficient to cover the stump. It is necessary to cut carefully round the bones, and to detach the muscles from their situations in the forearm, for they are connected closely to the bones and to the dense fascia, so that they do not retract like the muscles in the lower extremity; you must detach them, therefore, to some extent, in order to get a sufficient covering for the bones after the amputation.

Partial Amputation of the Hand or Foot.—It sometimes happens in the hand or foot that disease or injury attacks a considerable portion of the member, without actually rendering the whole useless. Scrofulous disease may be situated in the metacarpus, it may affect one or two bones of it, and not the others. Sometimes similar disease may be situated in the metatarsus, and not extend to the tarsal bones; it may be a disease of such a kind as to render necessary the immediate removal of the part affected by it, and yet there may be an advantage in limiting the excision to the diseased parts, because, in this way, you may leave the patient with a more useful limb than if you performed amputations of the entire hand or foot; a partial amputation of the hand or foot, therefore, may be advantageously practised under certain circumstances.

It not unfrequently happens that the hand is extensively injured. It may be shattered, or severely wounded, by the bursting of a gun or pistol; or it may be entangled in machinery, and dreadfully torn. Under these circumstances you may find that one or two of the fingers, or the

thumb, may have escaped, while all the rest may be so lacerated, and the bones so broken, that you cannot hope to restore them to a useful condition. You therefore take away the parts that are irreparably injured, and leave behind those which you may reasonably expect to restore; although it should be the thumb, or even the little-finger alone, the hand will be much more useful to the patient subsequently, than if all the fingers had been removed. You should divide the soft parts therefore, and cut through and remove the injured metacarpal bone, leaving those which have escaped injury. When you first see a part that has been extensively lacerated and mangled in this way, you are inclined to suppose that it will be necessary to remove it entirely; but you should carefully examine the case, and when you find that even a little finger has escaped, you should, if possible, give the patient the benefit that is to be derived from retaining that part.

In the foot, partial amputation has been performed; in the case, for instance, of scrofulous affection of the metatarsus, not extending to the tarsus. Here amputation may be performed in the middle of the foot, that is, at the articulation which connects the metatarsal to the tarsal bones. The operation is simple enough; you make an incision along the back of the foot a little in front of the articulation. It is a kind of flap operation, and the flap of the soft parts must be made from the sole of the foot, for, on the back, you have merely thin skin covering the bones, which will not answer the purpose. Then make an incision across the sole of the foot as much in front of the articulation as will enable you to take up a flap sufficient to cover the wound. When you have turned up the integuments on the bottom of the foot, and made the flap, you must divide the ligaments that tie the metatarsal to the tarsal bones, and remove the former with the toes. You then bring up the flap, and unite the wound by adhesive plaster, or by sutures. Partial amputation of the foot has even been performed, when certain of the tarsal bones are the seat of disease, between the astragalus and os naviculare towards the inside, and the os calcis and os cuboides on the outside. The line of junction between these bones is nearly transverse, about an inch in front of the ankle; and if you perform the partial amputation in this case, you would do as I have mentioned—make a transverse division along the back of the foot, detach the parts in the sole, and separate the bones at their articulation.

There is some doubt in my mind respecting the advantage to be derived from these partial amputations of the foot. The surface that is left for supporting the weight of the body, is so different from the natural surface of the foot, that perhaps it may be doubtful in certain cases, whether it is better than a wooden leg. I admitted a patient some time ago into the Hospital, who had undergone partial amputation at the tarsus with the metatarsus, and he found the condition of the stump so inconvenient—it was so liable to ulceration of the cicatrix, and the other inconveniences were so great—that he came to the Hospital to beg that I would amputate the leg at the usual place just below the knee, so that he might wear a wooden leg. I did this, and he found himself much better off after the second operation than after the first, for in truth, he had not been able to walk without pain during the two years that had elapsed since the first operation.

Amputation of Articular Extremities in diseased Joints.—In the case of disease of the joints, it has been proposed in certain instances to substitute for amputation of the limb, excision of the diseased extremities of the bones; to make such incisions as will lay bare the ends of the bones constituting the articulation, to saw through them and to remove them. This operation was first proposed by Mr. Park, a surgeon at Liverpool, a good many years ago, though I believe he is still living. He performed it at the elbow and at the knee-joint, and he stated that in both instances the limbs were afterwards of very considerable use to the patients. The operation has since that time been practised by others, but the results have not been sufficiently encouraging to lead to a very general adoption of it. Indeed, on the first proposal of such an operation, it strikes us as one not very likely to lead to useful results. In respect, however, to the elbow-joint, I believe that in some cases where it has been performed, the patient has retained a very useful degree of motion of the forearm, together with some mobility in the situation of the excised joint. The results have been most favourable in the case of the elbow-joint. In the case of the knee, however, the portion of the bone to be removed is so large, and the limb after the operation would seem to be so little capable of supporting the weight of the body, or of performing its functions, that we can hardly expect that it should be more useful than the ordinary wooden leg would be, after amputation of the thigh. I should observe, however, that this excision of the diseased ends of bones in other parts, which would otherwise have required amputation, has lately been performed by Mr. Crampton of Dublin, who has given a favourable account of the results. I should suppose that in order to repeat this operation with advantage, it would be necessary to have a case where you should be quite satisfied that

the cause of disease existed in the bone, and that the soft parts surrounding the joint had not suffered very extensively. It is only applicable to those cases where the soft parts remain in a tolerably sound state; and perhaps, under such circumstances, it might be performed at the elbow. I should observe, however, that I have never performed the operation, nor have I seen it performed, therefore I know nothing on the subject from my own experience.

Lithotomy.—I have next to speak of the operation of lithotomy; and I have made every effort to procure a subject, on which I might show you the operation, but in vain, and I must now consider it hopeless; I shall, therefore, proceed to speak of some of the points connected with the operation, and show you some of the instruments that are used in performing it.

I have already apprised you that I consider the *lateral* operation in which the neck of the bladder is divided, the external opening being made on one side of the perineum,—preferable both to the *Aigh* operation in which the opening is made above the pubis, and to the more recently-proposed *recto-vesical* operation in which the bladder is opened through the rectum; the latter indeed I believe has not been practised in this country, so that we know nothing of it here from actual trial. The lateral operation, in the ordinary run of cases, appears to me to be the most simple mode of making an opening into the bladder and taking a stone out of it; and at the same time it has this advantage over the high operation, that the incision into the bladder is made in a situation where there is no risk of wounding any very important parts. This is not the case in performing the high operation, for there you necessarily come very near to the peritoneum, and run the risk of wounding it, or of exciting inflammation in the cavity of the abdomen or surrounding cellular membrane. It appears to me that considerable risk is inseparable from the performance of the high operation.

Previous to performing the *lateral operation*, you secure the hands and feet of the patient in such a way that he may be easily held in the situation in which you place him, and that, at the same time, the perineum may be completely exposed. The confinement of the hands and feet, is rather for the purpose of procuring this exposure of the perineum than to prevent the movements of the patient, of which there is no greater danger in this than in other operations. To bind the hands and feet, you take one of these garters and double it in the way I now show you, pass the two ends through the noose that is formed, and put that round the wrist of the patient; then he grasps firmly with the hand the sole of his foot, under which you carry one end of the garter from the outside, and the other end from the inside, crossing them in that situation; you then bring them both up over the back of the foot and carry one on the outside, and the other on the inside round the ancle and wrist, so that they form a figure of 8. In this way the hands and feet of the patient are very effectively confined. Before doing this, however, you introduce into the bladder the instrument called a *staff*, which has the same form as the sound, differing from it only in having a deep groove along a part of its convex side, which serves as a guide for the cutting instrument that is to make the opening into the neck of the bladder. This instrument you introduce into the bladder, and feel the stone with it. And it is considered a *necessary* precaution that you should be sure of feeling the stone immediately before you proceed to the operation. It is not to be deemed sufficient to have felt the stone two or three days before. It is the striking of the instrument on the stone that affords the *only clear evidence* of its existence in the bladder, and you ought to have this proof immediately before you proceed to the performance of the operation. After having introduced the staff into the bladder and felt the stone, you have the hands and feet confined in the way I have described. The hair should be previously shaved off the perineum and parts about the anus, so that they may be completely clear. When the surgeon who operates has placed the staff in the position which he thinks best, it is to be firmly held there by the assistant who stands by the side of the patient. The position in which the instrument should be held during the performance of the incision through the integuments and adipose membrane, is not, however, of much consequence, provided its extremity is into the cavity of the bladder.

The first step of the operation then consists in making a division through the integuments and subjacent soft parts until the groove of the staff is exposed, and the parts immediately behind the bulb of the urethra are freely divided. If you are operating on the adult, the general rule to be observed respecting the direction of the external excision is, to make it about an inch in front of the anus, and then to carry it obliquely outwards and a little downwards between the anus and the tuberosity of the ischium. The wound through the integuments and external soft parts should be so situated and directed as to be as much as possible opposite to the wound in the side of the bladder, that there may be a straight passage for the forceps by which you are to seize the stone. The object, therefore, would be to have a completely parallel cut, a cut which should have precisely the same

direction in the neck of the bladder and in the integuments, for then you can introduce the forceps in a straight direction from without into the bladder. If you make the wound in the integuments more forwards or upwards than I have mentioned, then the course of the incision will be curved, because the wound in the bladder can only be in a certain situation; you have no choice as to that, and your business is to make your internal incision exactly parallel and opposite to it. Now, supposing this to represent the arch of the pubis (*referring to a diagram*), and this the opening of the rectum, the course of the incision will be about such as I now point out to you. It commences in the situation of the raphe of the perineum, about an inch in front of the anus, and extends a little outwards and downwards between the anus and tuberosity of the ischium, being about two or three inches in length. This incision is made with a double-edged sharp-pointed bistoury; the point of which you pass through the integuments deep into the perineum, directing it into the groove in the staff with the finger. Having carried the instrument in that way, you direct it outwards so as to make the incision in the way I have mentioned. Frequently however, particularly if the subject be fat, you do not cut into the groove of the staff at once, nor is it necessary to do so; you may be obliged to make two or three strokes with the scalpel; and then when you come to feel the groove of the staff, you put the nail of your fore-finger into it, and this serves as a director in making a free division along the urethra immediately behind the bulb. The reason why I particularly mention this situation as the part in which the opening is to be made, is this, that if you were to slit up ever so much of the canal in front of the bulb, it would not at all facilitate the extraction of the stone; such a division can therefore have no object. This division of the urethra terminates the first stage of the operation. Then having the groove of the staff thus completely exposed in the perineum, you adapt to it the beak of the instrument which you employ for making the opening into the bladder. Various instruments are employed for this purpose; and the one perhaps most commonly used, is called a *cutting gorget*. Formerly this instrument was made with a blunt edge, and employed only as a director for introducing the forceps into the bladder; but it struck some operators that it might be advantageously used for making the opening into the bladder itself; a sharp edge was therefore given to it, and hence it was called a cutting gorget. This gorget has a *beak* as it is called, which fits into the groove of the staff; and when you have exposed the groove as I have already described, you put the beak of the gorget into it, you move it a little backwards and forwards to satisfy yourself that it is in it, and you then take into your own hand the handle of the staff, which had before been held by the assistant. In the former part of the operation, you had required the use of both hands in order to make the incision into the groove of the staff, and to feel that you had got into it; but when you have got the beak of the gorget inserted there, you take the staff into your own hand, and move it in such a manner as to keep it in a certain position with reference to the axis of the bladder, moving the other hand with the gorget gently along, so that this may be confined to the groove of the staff, wherever it may be. Now you will immediately perceive, if you hold the staff in the axis of the bladder, and move the gorget steadily onwards, the latter will necessarily divide the urethra, the side of the prostate gland, and the neck of the bladder. That portion of the gorget which is situated to the right of the beak, has a cutting edge; it is in fact a knife of a particular shape, and in moving it in this way, you cut whatever comes in contact with it; thus you make a lateral division of the urethra, of part of the prostate, and of the neck of the bladder, to an extent corresponding to the breadth of the gorget. If you want to make a larger division, you must use a larger gorget; the lateral incision will always correspond to the breadth of the instrument. When you come to introduce the gorget, your object is to carry it as nearly as you can into the centre of the bladder; with this view, and that you may avoid wounding any parts that are not required to be cut, you take the staff into your own hand; you depress the handle, and thus elevate the point; by carrying up the handle, you make the groove side press against the inferior part of the bladder and the rectum; you must combine the movements of the gorget with those of the staff, and in order to give the beak of the former the proper direction, you must depress the handle of it when you depress the handle of the staff. When you have carried the gorget into the bladder, it then slips off the staff, and you feel that the two instruments are no longer in contact.

The affair of introducing the gorget along the staff into the bladder, is by no means a very difficult one; the only circumstance of difficulty is, that you have got to keep the two instruments in exact contact with each other, and you must do this without seeing them. When you are looking at them, nothing is more easy than to move them along in contact, but if you look another way and move them, you do not find it quite so easy; yet it only requires a little consideration and coolness to do it with the same facility in the body, as you might be

able to do it out of the body. You must bear in mind always the direction of the axis of the bladder; consider the importance of carrying the staff and the gorget in such a way, that their direction may correspond with the axis of the bladder; take great care not to place them in such a position that there can be any risk of wounding the rectum. The rectum lies very closely in contact with the bladder; and if you introduce the instruments in a direction in which the gorget will press downwards, it is by no means difficult to wound it. The rectum is connected to the bladder only by a little loose cellular membrane, and if you carry the staff so as to press on it strongly, and carry in a sharp cutting instrument upon it, under such circumstances it may very easily be wounded.

If you use the gorget, I do not know that you can employ one of a better form than the one I now show you, in which the cutting edge is directed a little outwards and downwards. It is not quite lateral, but it is a little inclined downwards from a straight line, according to the direction of the instrument.

There are other modes of opening the bladder besides cutting into it with the gorget. You may use a knife for this purpose; in truth you may cut into the bladder with the same knife with which you make the division of the integuments, and expose the groove of the staff; you may carry the point of that knife onwards into the bladder, although it is not very well adapted for the purpose, for the sharp point of the knife would stick against the groove; therefore a beak at the end of the instrument is better, and if you use a knife, it will be convenient to have a knob at the end of it, in order that it may be easily carried into the bladder along the staff. This is an instrument of that kind—a long slender knife, with a beak, and this was employed by Mr. Thos. Blizard, surgeon to the London Hospital, an operator of great celebrity and dexterity. The beak of this instrument slides along the groove of the staff, and thus you carry it into the bladder, and divide the neck of it and the prostate gland, by moving the instrument a little downwards and outwards. The gorget makes the cut as it enters the bladder; but this narrow knife cuts in coming out. The gorget necessarily cuts to a certain breadth as you put it in, owing to the width of the cutting edge; but if you employ an instrument of this kind, the breadth of the cut depends on the mode in which you move the instrument. This is Mr. Blizard's knife, adapted for the purpose. It should be longer, broader, and considerably stronger, than what I now show you, which is the instrument for the young subject.

Various other knives and gorgets have been invented by different persons, who have fancied that some particular construction or shape of the cutting instrument would obviate the difficulties which they met with, but the multiplicity of gorgets and knives, and other instruments for cutting into the bladder which now exist, is a clear proof that an instrument that will go into it at all events, is not yet discovered; indeed I believe that those who have been engaged in the research, are satisfied that the success must depend on the hand that directs, and not on the instrument itself. There is no doubt that a person who has a good knowledge of the parts and organs that are to be divided, may make an opening into the bladder, and extract the stone with almost any instrument that has been devised; and I believe that without that knowledge, none of the instruments that exist will enable a person to do it very safely and properly. For my own part, I have always been in the habit of using that instrument which is called *Blizard's knife*. I do not pretend to say that it is preferable to all others, but at all events it is simple enough, and the operation is easily performed with it. I should be inclined either to use it, or that kind of gorget which is now sent round for inspection. I should say to persons who have not practised the operation much, nor consider very accurately the parts, that the gorget may be the safest instrument in their hands. It cuts the prostate and neck of the bladder in a determinate extent, and is, therefore, a more certain instrument than the knife; but for my own part I prefer the latter.

Then having made the incision of the prostate and neck of the bladder, you introduce your fore-finger along the staff into the bladder, and feel the stone with the end of your finger. Having done this, you remove the staff from the urethra, and introduce the forceps along your finger into the opening you have made; then expanding the blades, you move them about, and on bringing them together, you find that you have included between them the stone, which you have now only to draw out of the bladder. When the stone exceeds a certain size, you occasionally find considerable resistance to its extraction, so that before you have performed the operation a few times, when you begin to draw the stone out, finding that it does not come away easily, you begin to suppose that you have got hold of a piece of the bladder; but the truth is, the sides of the bladder, the prostate, and the soft parts situated between the bony arch, necessarily oppose a resistance to the extraction of the stone. You have only got a certain space between the bones for the removal of the stone; this space is occupied by the urethra, the muscles that surround

it, the adipose and cellular substance and other soft parts, and you have to draw it through an opening you have made in half that space; you have to extract a stone perhaps of considerable size, with the addition of the thickness of the blades of the forceps. You will immediately see, therefore, that you cannot make an opening in those parts sufficient for a stone beyond a certain size to come out; there is not actual mechanical space for it, even if you could get the whole of that which exists between the bones. I have tried all kinds of instruments on the dead body, so as to see the mode in which the prostate and neck of the bladder have been divided, and I have never found that the incision has gone further than the prostate. Now if you compare the size of the opening that can thus be made with the ordinary size of stones found in the bladder, you will immediately see, that in dragging a stone through an opening there, you must accomplish the removal of it by considerable distention and partial laceration. No doubt the violence that is thus offered to the parts, and the inflammation which it causes, are the principal sources of the mischief which takes place afterwards, and the source of the danger to which the patient is exposed; but it is inseparable from the operation.

In drawing the stone out, when you come to experience resistance, you should hold it firmly, move the forceps from side to side, then pull it gently, performing this distention and laceration of the soft parts with as little violence as will just answer the purpose; move the forceps from side to side, then stay a little, giving an opportunity for them to yield without any very violent or forcible proceeding. You should observe also, in doing it, to draw the stone as much as you can towards the lower part of the pelvis, where the interval between the bones is the widest. If you draw upwards, you attempt to extract the stone through the narrowest part of the arch, and you will find that the forceps in a certain state of extension will not come through this part, while, if you depress the handle, they will come with great facility through the lower part of the arch. The patient is laid on his back during the operation, therefore in drawing the stone out, you should draw almost perpendicularly downwards towards the ischium; if you elevate the handle of the forceps, you bring the stone against the pubis, and opposite to the narrowest part of the bony arch.

If you have removed one stone, you may introduce your finger, and see if there are more; and if the stone should have given way, and broken under the operation into fragments, you may remove a part of those; you may remove more of them, and still find that there are others remaining behind. Under such circumstances, it is expedient to throw in a little warm water into the bladder with an ordinary glyster-pipe, so as to scour it out.

This then is the most common way of performing the lateral operation for lithotomy; it is not, however, invariably or universally adopted; for instance, of late, some gentlemen have preferred using a straight staff, or a staff very nearly straight, with a slight curvature at the end of it. If this staff is used, it is necessary to take great care to hold it in such a way, that it may be directed towards the axis of the pelvis; for if you carry it more depressed, the cutting instrument would be very likely to wound the rectum and the neighbouring parts. If the straight staff is used, the handle must be brought considerably down, indeed nearly parallel to the thighs of the patient, to admit of the cutting instrument being carried safely into the bladder.

There is an instrument which has been frequently employed for making an opening into the neck of the bladder and prostate gland called a *bistoury*, or by the French *lithotome caché*; it is contained in a sheath, formed with a beak at the end, which can be introduced into the bladder along the groove of the staff; then, when it is in the bladder, you force it out of the sheath by a kind of lever, and draw it straight out, so that you make a horizontal incision through the prostate and neck of the bladder. Here you can make an incision limited in extent as you do in using the gorget, but, in this instance, you make it in coming out, instead of in going in. The extent to which the blade starts out of the instrument is determined by the position of the screw in the handle; if you carry the screw to a short extent, then the blade only comes out a short way; if you carry it further, then you increase the extent to which it comes out. This instrument has been a good deal used in France, and I believe sometimes in England, and it is not a bad instrument. It comes out with great facility; the knife being sharp when you draw it out, it passes so easily, that you can hardly fancy that you have made a cut with it.

A mode of proceeding has lately been proposed, and extensively practised by the Baron Dupuytren, which he calls the *bilateral* operation; it is executed by means of a double *lithotome caché*, which, when introduced into the bladder, appears as a simple instrument. In this method the first incision is made in a different direction from the external opening which is made in the lateral operation; supposing this to be the anus [*referring to a diagram*], the direction of the incision is semicircular in front of the anus, going equally on the two

sides of the raphe of the perineum; in the lateral operation, you commonly cut only on the one side; but, in this bilateral operation, the incision is semicircular, the ends of it being turned towards the anus, and the convexity towards the penis. The groove of the staff is exposed in the situation which I have before mentioned, just below the bulb of the urethra, and then this instrument is passed in by means of the staff into the bladder. When it is there it is turned, so that the convexity is directed upwards, and the concavity downwards; then you press upon the two handles by which you open the instrument—it is exactly similar to this simple lithotome caché, except that there are two blades, one on each side, which can also be limited in the extent to which they start out, and which make an incision through the neck of the bladder and prostate on each side as you withdraw them. This operation has the advantage of making a freer division of the prostate and neck of the bladder—of making a larger opening than is usually made for the extraction of the stone. It might be usefully employed, where we have reason to expect that the stone is large. You can readily understand that the stone will come out more easily through an incision of this kind, than through one of only half the extent. It has also the further advantage, that the opening into the bladder is in the middle line, so that although it is beyond the usual size, it is not extended in any direction where an artery is likely to be wounded. In the middle part of the perineum the vessels are small, and, in fact, there are no large vessels there, except along the inside of the ramus of the ischium and pubis, where the trunk of the pudendal is found, and there is no risk of wounding that or any important vessel by this incision. I believe this bilateral operation has been found to answer extremely well in the hands of Baron Dupuytren, but like the recto-vesical operation, it has not been practised in this country. In cases where a large stone is expected, where the prostate is likely to be enlarged, and where you want a freer incision than ordinary, I think this operation might be employed with advantage.

After the operation for lithotomy, the urine flows out through the wound—the whole of it passes in that direction for a certain time; but in proportion as the wound heals, it begins to pass through the urethra, and to take its natural course. The patient sometimes experiences considerable uneasiness after the operation, and before the urine begins to flow through the wound; the sides of it, perhaps, are agglutinated by the blood that may have been discharged; he wants to make water, and cannot pass it; the bladder becomes distended, and very great uneasiness is the consequence, until the accumulated water forces a passage for itself. This inconvenience has been so considerable, as to induce some surgeons to introduce through the wound a small canula, which may give a passage to the urine, even if the sides of the wound should become agglutinated. The length of time that elapses before the urine passes by the natural channel, differs in different instances; generally, provided every-thing has gone on favourably, some water will begin to pass by the natural way in about a week; and in children it will begin to come by this way still earlier. In the last instance in which I operated on a child in the hospital, he began to make water on the third day; and I think I recollect one case where it came through the penis from the time of the operation, none passing through the wound, the sides of which, I fancy, adhered closely together. In an adult it generally begins to pass in a week from the time of the operation, and at the end of a fortnight nearly the whole of it will pass through the penis.

The after treatment of patients who have undergone the operation of lithotomy is often a matter of considerable importance. Inflammation of the bladder may come on, the cellular membrane surrounding it and the peritoneum may inflame; in the latter case active antiphlogistic treatment, the free abstraction of blood, the free application of leeches to the abdomen, etc., are what you have to depend upon. There are some few points remaining which will form the subject of another lecture.

Lecture LXXXIX.

(Conclusion of the Course.)

Lithotomy in the Female.—Extraction of Extraneous Substances from the Female Urethra, and from the Rectum.—Staphyloraphe.—Congenital Malformations of the Female Organs of Generation, and of the Anus.—Club-Foot.—Preternatural Connexion between the Fingers.—Supernumerary Fingers.—Counter-Irritation.—Acupuncturation.

Lithotomy in the Female.—The operation of lithotomy is very rarely necessary in the female. In the first place calculi are uncommon in the female bladder. If calculous concretions descend from the kidneys into the bladder in the female, they pass out through the urethra, the short course of which, and its large size, are particularly favourable to their escape, so that it very seldom happens that any nucleus remains in the bladder of a female, so as to constitute the source of a future calculous concretion. Again, the female urethra is susceptible of

dilatation to a very great extent—to a much greater extent than you might have expected *a priori*. You can enlarge the female urethra, so as to pass in your fore-finger, the thumb, or even a couple of fingers, and, consequently, so as to admit of the introduction of a pair of forceps into the bladder, with which a stone of considerable magnitude—a stone, we will say at least, as large as the end of the thumb, and probably as large as the second joint of the thumb, can be grasped with the forceps, and extracted without any incision. Mr. Weiss, the ingenious instrument-maker in the Strand, has devised an instrument with two branches that can be introduced into the urethra, be expanded by the mechanism of the instrument, and thus, within a short time, a sufficient dilatation can be effected for the introduction of a pair of forceps, and the extraction of a stone of a large size. In the female the operation is very rare, therefore; and if, in any case, it becomes necessary, it is very simple. A straight sound, and an incision made by any common knife carried along its groove in the direction of the lateral and inferior part of the urethra, will divide the canal sufficiently for the purpose of the operation.

Extraction of Extraneous Substances from the Female Urethra.—The same practice of dilating the urethra may be had recourse to in those instances in which extraneous substances have been introduced into the bladder through the urethra, and in such cases you may adopt just the same method for extracting those that you would for extracting a stone from the bladder.

I have already had occasion to speak to you respecting the introduction and removal of extraneous substances in various parts of the body. I have spoken to you of this as regards the eye, the air-passages of the lungs, the œsophagus, and now with respect to the female urethra.

Extraction of Extraneous Substances from the Urethra.—I may make to you the same observations respecting the rectum, as with regard to the female urethra; that is, in the case of extraneous substances either introduced from without, or which have descended from the intestines, that you can dilate the anus to a size that will admit of the introduction of several fingers, so as to extract with great facility any body that may be lodged in the rectum. When you introduce a finger into the anus, you seem to pass through a tube of small size barely admitting of its introduction; but you find that, after it has been a short time there, the resistance made by muscular contraction diminishes, and that it is very loose, so that you can introduce a second finger, very soon afterwards a third, and perhaps a fourth, and thus dilate the rectum in a manner that would admit of the introduction of forceps of considerable size, or any instrument you might be desirous of using for the purpose of extracting the extraneous substance.

Staphyloraphe.—I have had occasion to speak to you respecting the malformation of the lip which constitutes hare-lip. A similar malformation is sometimes seen of the palate; you may either have a fissure extending through the bony and the soft palate, or the former being entire, you may have a fissure of the latter alone. Where the fissure extends both through the bony and the soft palate, we cannot adopt any means of removing the deformity. You cannot bring together the sides of the bony fissure, and if you cannot do that, it would answer no purpose to unite the fissure of the soft palate. When the fissure, however, is of the soft palate alone, it is possible to perform an operation which, in respect to its principle and some of its details, is similar to that which is performed on the hair-lip. You may cut off the edge of the fissure so as to bring the margins into the state of a recent wound; you can draw them together by means of sutures, and thus you can unite the fissure of the soft palate. This operation has been performed several times in France, by M. Roux, the surgeon of the Hospital called *la Charité*, a very dexterous operator, and he has described it in a small pamphlet called *Staphyloraphe*, that being a compound Greek term denoting suture of the palate. He seems to have met with a great many of these cases, and to have met with many on which he performed the operation for removing the defect. I have seen cases of this description, but they have been under circumstances in which neither I have thought fit to advise nor has the patient been willing to submit to an operation. The consequence of the defect is chiefly observable in the voice of the individual; it produces an imperfection of the voice—an imperfection sometimes very considerable. The same kind of imperfection is produced in an individual who is unfortunate enough to lose a considerable part of the palate by ulceration. This imperfection, however, is not very great in all instances, so that there are cases in which persons are not willing to undergo a painful and troublesome operation formed for the purpose of getting rid of so slight a deformity. I have, therefore, not seen staphyloraphe performed, and have no observation to make arising from my own experience on the subject. One instance in which I met with a fissure of the palate was in a female who came to the Hospital, asking for advice with respect to a disease of the eye. In the course of examining her I observed that her utterance was rather peculiar, in the answers she

made to my questions; there was a peculiarity in the sound of her voice, and a singular movement of the ala nasi in speaking, which attracted my attention; and as they were somewhat similar to what I had seen in cases of fissured palate, I asked her if she had any-thing the matter with the roof of her mouth. She said, No. I asked her if ever she had any-thing the matter with it. No, she said. However, I looked into the throat, and there I found a fissure completely extending through the soft palate and dividing it into two parts, but the imperfection in this woman's utterance was not very great, and I therefore did not think it worth while for her to undergo so painful and disagreeable a proceeding on account of it; for according to the description of M. Roux, and as you may easily conceive, to get rid of the imperfection by cutting the edges and bringing them together by sutures, a good deal of manipulation must take place, which must be very unpleasant; he states that the patient ought to be kept with the mouth shut, and without swallowing either liquids or solids, or even speaking, for three or four days; so that it is not a very pleasant proceeding: I have seen two other instances where by adjusting a kind of apparatus an artificial palate has been made, and as much improvement in the articulation has been produced as could have been effected by the operation. I should therefore apprehend that the cases are very few in which this operation can be required. And they must be cases where the fissure is confined to the soft palate, and does not extend to the bones. In the majority of these cases, however, the deformity exists both in the bony and in the soft palate.

In female children there are sometimes *congenital imperfections* consisting of an unnatural union of the *external organs of generation*. The labia are sometimes united by their edges, so that the opening of the vulva appears to be obliterated. Perhaps there is an opening sufficient just to admit a small probe either at the upper or lower end, and through this the urine flows out; sometimes the *nymphæ* adhere in this way. Now these parts are in general only superficially united, and as the skin is very tender there, you can usually separate them with your fingers, or at any rate, by the introduction of a probe or director.

There sometimes occurs a more serious malformation of the internal parts close at the entrance of the vagina, which has commonly been called an *imperforate hymen*, and where this membrane completely closes the passage, instead of having at its upper extremity a semilunar opening into the vagina. The existence of this malformation becomes apparent at a certain period of time—after menstruation has commenced; for as the menstrual fluid cannot flow out, it gradually accumulates in the vagina and uterus, and ultimately enlarges these parts in such a way as to give the female the appearance of being pregnant; and this is the more deceptive, inasmuch as the enlargement of the abdomen is accompanied with the absence of the usual monthly discharge. We cannot be surprised therefore, that persons have occasionally mistaken the cause and nature of such enlargement, and supposed females to be in a state of pregnancy who could not possibly have got into that condition. In these cases, where the existence of the malformation has been ascertained, the remedy is easy: you have to perforate the closed hymen—make a straight opening into it with a double-edged sharp-pointed knife, and in order to give to it sufficient dimensions so as to save the female any inconvenience afterwards, you should make a crucial incision, and then there will be no danger of the edges of the wound re-uniting afterwards.

Atresia Ani.—It sometimes happens that children are born without any anus; the formation of the alimentary canal is perfect, except that the large intestine is closed at its extremity. In some instances you find an external appearance just like an anus, and the only difference is, that it is closed; it seems as if the anus were formed—that all the essential parts existed, but that the intestine is closed at its extremity. In these cases, the bowel sometimes terminates by a blind end a little way beneath the skin; sometimes at a considerable depth from it; sometimes it may open into the vagina, or into the urinary bladder; there are varieties of kinds of malformation of this. Now in these various cases, the only one in which we can render essential service, is that in which the formation of the alimentary canal is complete, with the exception of its external opening; there we can make an opening by means of a double-edged sharp-pointed bistoury carried into the rectum, and by the daily introduction of the finger or a bougie, as the edges of the wound heal, we may prevent agglutination till the parts have cicatrised. There are persons who have had an anus thus formed, who have grown up, and have had the full command over the sphincter muscle, and done as well as those who have had a natural anus. In the instances in which we cannot see any anus, and where we are obliged sometimes, from the sufferings of the child or the importunities of parents, to dissect cautiously into the extremity of the bowel, if we see a part of the blind end of the rectum distended with meconium, we may puncture it, and the child must take its chance of the future event, whether it will prove a permanent anus or not. In the more serious malforma-

tions of this sort, if we do not find the anus or any appearance of it, within two inches from the natural situation, it is not advisable to attempt to penetrate any further.

Club-foot.—The kind of malformation which constitutes what is called *club-foot*, can only be removed by mechanical contrivances; that is, by the adaptation to the foot of such mechanism as will keep it in its proper bearings, in respect to the bones of the leg, and these are applied with the greatest advantage in the earliest periods of life. At that time, the bones of the foot are merely cartilaginous above, they easily give way under the application of proper mechanical means, such as can bring the foot and leg into their proper positions, which is not the case at a more advanced period of growth when ossification has proceeded further. The management, however, of these cases generally falls to those fabricators of mechanical apparatus who make it their particular object, and I shall, therefore, say nothing further on the subject, than to urge the necessity of using the requisite means at the earliest period while the bony structure of the foot and ancle-joint remain in a soft and yielding state.

Preternatural Connexion between the Fingers.—There are instances in which the fingers are preternaturally *connected* together at birth, *web-fingers*. The deformity is easily removed by separating the parts.*

Supernumerary Fingers.—There are instances of supernumerary fingers,—an extra finger or thumb, which are sometimes so connected as to be inconvenient, and to require removal.

Counter-irritation.—Respecting the mode of accomplishing the various means of counter-irritation I have little to say, for really the making of issues, setons, &c., is so familiar that it is hardly necessary for me to enter professionally on the subject. I will only observe that an issue may be made either by pinching up a part of the skin,—pushing a knife through, then inserting peas or glass beads in the wound; or by the application of some caustic substance, especially the potassa fusa—you may take a stick of pure potash, rub it on the surface of the skin, and thus destroy it in a sufficient extent to admit of the introduction of peas or glass beads, or any other substance you may adopt, for the purpose of keeping the wound open. Perhaps a less objectionable mode of using the caustic, is in the form of *paste*, when the potash is mixed with as much soap as will make it like a pretty firm paste; this is spread on a thin rag and placed on the part previously surrounded with sticking-plaster, to prevent the action of the caustic extending further than is requisite; cut a hole of the size of the intended issue in a piece of doubled plaster, lay this on the part, and then fill the open space with the plaster of the soap and caustic, let this remain for four or six hours, then remove it, cover the parts over with a poultice, and wait for the regular suppuration of the eschar which is thus produced. The issue thus made, may be kept open easily; the more ordinary way is by inserting peas, beans, or glass beads, into the wound, confining them by means of sticking-plaster, and renewing the dressing daily. This method is sometimes a painful one; a difficulty is experienced in keeping these substances in, and maintaining the ulcers of the requisite extent. You may, however, keep open the issues for a considerable time without these substances, and merely by rubbing the surface occasionally with the caustic potash. There are two modes in which *setons* may be made. In the ordinary way you use a broad, flat pointed knife with an eye at the end of it called a *seton-needle*, and in that you can place a skean of silk, you then press the seton-needle armed with the silk, cut off the needle and confine the silk in its situation. Now, the discharge produced from the seton lodges in the silk and becomes offensive; and a seton kept up in this way is very apt to be attended with a disagreeable smell; so that on the whole a more cleanly way is to employ *seton tapes* made with elastic gum, which are more convenient and less offensive, as they are easily sponged clean every time you dress the issue. This tape is passed just in the same way as the silk, except that the end of the needle is formed somewhat different, with a slit instead of an eye to receive it.

It is hardly necessary to say any-thing about the mode of applying the actual cautery, for it is so little used in this country, that surgeons hardly like to propose it, and I fancy that patients would hardly be inclined to submit to it. I do not suppose that the actual cautery is more powerful than some things we are in the habit of using, but people are guided a good deal by the general adoption of things of that kind, and they will object very positively to one remedy, while they will have very little objection to bear a much greater quantity of pain in another way. The actual cautery might, however, be employed with advantage in some cases. It is a powerful agent;

* It is an established fact that there is scarcely any-thing in surgery more difficult than the cure of this deformity, from the very great tendency which the fingers have to reunite, mere division of the uniting medium is never sufficient, one of the most efficacious methods, is that of forming a flap of skin between the fingers at their base as described in a previous number of THE LANCET.—ED. L.

and is perhaps too much neglected in this country, in the same degree as it is too much employed elsewhere. The *moxa* is merely a species of the actual cautery. The mode of applying it is very simple: portions of cotton may be rolled up compactly in a cylindrical form of various sizes; one of these prepared cylinders is to be held to the part, having been previously lighted, and a current of air directed upon it from a blow-pipe, till the skin is entirely destroyed; till in fact it is burned black, as it were, into a perfect coal. The action of the fire is not precisely confined to the spot immediately covered by the moxa, for you will see that, to a considerable extent, redness is produced of the surrounding skin, which is wrinkled and drawn together by the contraction of the eschar. I consider certainly, the application of moxa to be a more powerful remedy than the issue or seton. This portion of skin which is destroyed, is gradually separated, and an ulcer is left, resembling that which is produced by the caustic; but the application of the fire produces a more powerful derivative or revulsive effect than that of the potash. Although the moxa is similar to the seton or issue, yet I consider it on the whole to be more powerful.

Acupuncturation.—I have nothing particular to say to you respecting acupuncturation, an operation that has been derived from the Chinese or Japanese, to whom I suppose we are not indebted for the invention of any other kind of operation; but in those countries, the *pricking* of parts of the body with *needles*, is one of the most general and familiar operations. It has been tried lately in this and some other countries of Europe; and great benefit has sometimes been derived from it in obscure painful affections, in rheumatism, and in other similar cases where probably there has been no serious disease. The application of the remedy is very simple; it merely consists in introducing the point of the needle by a rotatory motion to a certain depth. It was some time ago considered a fashionable practice, but I believe it has now gone out of fashion; we do not hear much now of diseases being treated by acupuncturation.

[The learned lecturer here digressed for some time, and apologised for the imperfections to be found in the lectures. Mr. Lawrence then concluded in the following words.]

Gentlemen, before taking my leave of you allow me to say to those who may have concluded that portion of time which they mean to devote to their studies in London, that I trust they will not consider their education completed, and that their further attention is not required to those points of professional knowledge which are necessary to qualify them for entering into practice. It is impossible, I conceive, for any person fully to convey to others by oral discourses the knowledge he may happen to possess on a particular subject. You cannot convey knowledge from this mind to that, as you can pour fluid out of one vessel into another. The acquisition of knowledge, so far as it can be of real service in a profession like that of medicine, requires great activity of mind. In listening to lectures and other discourses the mind is passive; you can hear easily enough what is delivered; but in order to qualify you for the duties of the profession you must actively exert your own faculties, and therefore your education only begins at the time when most persons think it is finished. I conceive, indeed, that the only valuable information a man obtains, is that which he himself procures. It is the direction of his own energies to the subject in question which alone can strengthen those powers of mind which lead him to gain the kind of knowledge that will be of real use to him afterwards for practical purposes in his profession. In this respect we may really say that the education of a member of the medical profession is never at an end.

[Mr. Lawrence retired from the theatre amidst the warmest plaudits of the students.]

CASE OF

POPLITEAL ANEURISM.

The following case of popliteal aneurism was related by Mr. Lawrence to the students at St. Bartholomew's Hospital, previous to the commencement of his last lecture (published in our present number) delivered on the 30th of April:—

I had occasion just now, to see in consultation a case of popliteal aneurism, and in examining it I was detained a little longer than usual. It was a case of particular interest, and I shall take the liberty of mentioning to you the points of it. The tumour had attained a considerable size, and presented the usual characters of the disease. The individual in whom it occurred was desirous of putting off the operation, and the gentleman who saw the case, in consequence of some other business, had been absent from him for a short time; suddenly a considerable change took place, the tumour enlarged, the whole limb swelled, and yesterday evening and this morning it had become cold and hard, so that the commencement of gangrene was anticipated. When I saw the patient this evening the coldness had gone off, and the natural temperature of the limb had returned, but the

whole of the thigh and leg were excessively distended, and for the greater part very firm. There was an immense tumour in the ham quite incompressible, and no pulsation whatever could be distinguished in it. These circumstances had led to the idea that the circulation through it had come to an end, and that those changes were going on which were likely to lead to a natural cure. In this case I did what the experience of other cases taught me the importance of, that of applying my ear to the tumour in the situation of the artery, and I heard the circulation going on distinctly, and that particular whizzing noise called by the French *bruit de soufflet*, from its resembling the impulse of air in the narrow mouth of a bellows, and produced by the blood flowing through the aperture of the artery into the aneurismal sac. Now there could be no difficulty in coming to the conclusion in this case, although they had supposed that the circulation through the tumour had discontinued, that it was going on and probably increasing the swelling, and keeping up all the dangerous symptoms under which the patient had laboured; consequently, the ligature on the artery will be the proper course of proceeding in that case; it is a point on which the successful treatment of the case essentially depends. I apprehend the result will be that the artery will be tied; however, it is not a case that is under my own care.

THE LANCET.

London, Saturday, Sept. 18, 1830.

THE contest for the vacant office of Coroner for the County of Middlesex, is proceeding in a manner that is calculated to afford the most unbounded satisfaction to every unprejudiced mind in the kingdom. From what we have recently observed, we may state that the question, connected with this branch of medical reform, was never till now fairly before the public. The discussion before the hustings at Clerkenwell must be productive of infinite benefit. We always considered that it was only necessary to place the matter in its true light before the community to obtain, from the almost unanimous voice of the people, the opinion that attorneys are not qualified for the office of Coroner. The feeling of the people is now of the most decided character; and had not Mr. Baker obtained the promises of a considerable number of the Freeholders before the necessity for a medical Coroner was understood, he would not probably have polled one hundred votes. As it is, he has not the least chance of success; but the cause requires and demands the most active exertions, collectively and individually, on the part of the independent medical practitioners residing within the county. It is *their* cause, and their reputation; their happiness, their success in life, and even their existence in a great measure, depend upon the appointment of at least some medical judges. The poll will close precisely at four o'clock on Monday next; meanwhile it is hoped that every surgeon, physician, and apothecary, will exert himself—will explain to the Freeholders with whom he may happen to be acquainted, the vastly-important medical functions which a Coroner has to discharge. Should the Freeholders coincide with him in opinion that such duties cannot be properly executed by a man unlearned in medical science, it is hoped that they will then be induced at once to proceed to the poll. During the whole of the present contest nothing in the shape of argument has been advanced against the claims of medical candidates, and when on Monday last it was proposed to the immense assemblage at Clerkenwell that the persons present should decide by show of hands, whether Mr. Wakley or Mr. Baker were the fittest person to hold the office, it was computed that not less than twenty-five thousand hands were raised in favour of the former gentleman. The public appear to understand the question most perfectly; even the humblest individuals discuss the subject in a most rational and intelligent manner. The facts they adduce for the appointment of medical coroners are strong and decisive, and the reasoning arising from those facts is clear and conclu-

sive. It is seen in all quarters that such appointments would effect a thorough reform in the method of educating medical students, and in the mode of governing our Hospitals. The benefit to the students, and the protection to the patients, which must result from placing medical men in the office of Coroner, are universally and unequivocally acknowledged. The public now know, for the first time, the disgraceful manner in which the duties of the Coroner's office have been performed by attorneys; indeed, it is a well-ascertained fact, that in the eastern district, the Coroner seldom viewed the body on which it was pretended the inquest had been held. On some occasions there were inquests upon bodies that were *not* dead, and Mr. Fuller of 56, Old Street Road, Shoreditch, alleges that three inquests were held upon the body of Catherine Moody, in the London Hospital—that a verdict of manslaughter was *three* times returned against a person who had pushed her down some steps, and that, even after the last verdict, one of the Jury called at the Hospital, and saw the old lady devouring very comfortably some fine native oysters! Much as we have written on the office, our animadversions, it appears, have fallen far short of exhibiting adequately the ignorance and delinquencies of non-medical coroners. We have acted, however, as pioneers, and cleared the road for the public; no question can now be better understood, and already in seven days have two thousand five hundred and twenty-nine of the freeholders of Middlesex recorded their votes in favour of the medical coroner. As it is desirable that the medical candidate should win by a triumphant majority, it is hoped that every medical practitioner residing in the county, who feels at all interested in the cause (and where is the man who does not?) will canvass his neighbours, and will take, as early as possible, one, two, three, four, or more freeholders to the poll; always recollecting that the poll must close, according to act of Parliament, at four o'clock next Monday afternoon.

State of the poll, Thursday, Sept. 16th:—

Mr. Baker.....	2763
Mr. Wakley	2529

On the Motions and Sounds of the Heart. By Dr. Corrigan.

The name of Dr. Corrigan is already familiar to our readers, and with them we have no doubt that he has acquired considerable reputation. In the paper now before us, he has put forward novel and extraordinary doctrines, startling in their kind, and of immense importance, as far as regards the decision which the profession may ultimately form of their merits. In the present notice we shall entirely restrict our observations to the foundation on which his superstructure has been established, as by the stability of this alone, is the strength of the entire construction to be estimated. We shall also have to advert to some critical notices which Dr. Corrigan's pamphlet has already elicited, for the double purpose of marking another stage in the progress of corruption in medical literature, and of elucidating the issue of the questions we shall have to examine.

It is by no means the least remarkable feature in the medical book-making of the present day, that notwithstanding the polygraphic mania which so universally prevails, yet originality of subject or ideas is unprecedentedly rare. Men write on every-thing and on nothing, or in general only seek for an apology to stereotype their names in the columns of a periodical, and thus, though the quantity manufactured is enormous beyond measure, still the quality of the materials is of the most contemptible description. That numerous evils should spring from this epidemic, can scarcely be a matter of surprise, and of these, perhaps one of the most important is the effect produced on the majority of reviewers, by the influenza contagion of the dulness they are accustomed to examine; habituated to rust and rubbish, they gradually conceive antipathies to everything not so stupid as themselves, and especially they become the enemies of originality, in order to save themselves the trouble of properly investigating the merits of a

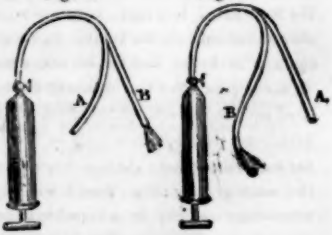
proposed innovation. Scarcely were Dr. Corrigan's ideas committed to type, before his original sin excited the fury of these guardians of antiquity; "liar," "jackanapes," and "fool," resounded on every side, and in the eagerness to prevent his intrusion on their venerable domains, professional dignity was totally forgotten, and the prerogative of criticism degraded to an unexampled condition of moral prostitution. Of all the knaves and fools who endeavoured to crush Dr. Corrigan beneath the accumulation of the filth they showered on him on every side, one only had the cunning to disguise himself in the show of candour and argument, and to him in the sequel we shall direct our especial attention.

In the great majority of the modern schools of physiology, the doctrine is promulgated, that the impulse of the heart as felt at the ribs, is occasioned by the striking of the left ventricle against them, during the time that it contracts and expels its blood into the arterial system; that, consequently, the impulse at the side, and the pulse at the wrist, temple, or elsewhere, take place at the same moment, and that any deviation from this synchronism is indicative of disease. So far the teachers of physiology and pathology have, since the times of Hunter, coincided in opinion; but innumerable difficulties and disputes arose as to the mode of explaining the apparently paradoxical position, that the heart or left ventricle should rise in the chest and strike the ribs, at the moment of its contraction into the smallest volume it is susceptible of assuming. Of all the hypotheses advanced, none was so widely adopted as that proposed by Hunter, amplified by Bostock, believed by Laennec, and generally assumed as sufficient by the most eminent medical authorities, namely, that the aorta being a curved tube, tends to strengthen itself when suddenly injected with blood; that its loose end must be consequently elevated, and thus that the heart is tilted up against the ribs, its impulse being assisted by the recoil of the compressed fluid against the internal surface of the left ventricle.

Such, we repeat, has been, and such is, the general opinion of the nature of this occurrence; and it is here that we find Dr Corrigan attempting his first innovation. Dissenting from the statement in physics, that the injection of a fluid through a curved tube must necessarily produce the *elevation* of its loose extremity, he brings forward a simple, and, as he considers, most important experiment, to prove that it is the reaction at the mouth of the discharging orifice which is productive of motion, and that, consequently, the position of the orifice where the reaction takes place, must govern the direction the injected tube ultimately assumes. A correct understanding of this proposition is of material importance to the rest of the inquiry; we shall therefore quote Dr. Corrigan's description of the experiment, and the diagram by which it is explained:—

"To demonstrate the fallacy of the assertion made by Bostock, Magendie, Hunter, Laennec, and, indeed, by all physiologists who have supported their opinions, take the ordinary stomach-pump, with a long curved elastic tube attached. Inject water forcibly through it, and you will find, that where the orifice is curved in towards the pump, the extremity of the tube will move outwards, or the tube will tend to straighten itself. If the orifice be pointed outwards or away from the pump, the loose end will move inwards, or the curve will be increased. When the orifice is turned to the right or the left side, the tube will move in the opposite direction. These movements of the tube prove to demonstration, that the straightening of the curve, where it does take place, instead of being independent of, is altogether governed by the direction of the discharging orifice. The accompanying diagram will explain my meaning better than words."

Fig. 1. *Fig.* 2.



"*Fig.* 1. *A* represents the tube with its orifice turned inwards. *B*, the position which the same tube assumes from the reaction generated at the discharging orifice.

"*Fig.* 2 represents the experiment reversed; *A*, the tube with its extremity or discharging orifice turned outwards. *B*, the same tube driven in, or the curve increased, by the reaction of the fluid rushing out."

We shall not examine here into the validity of this experiment, but from it, taken together with the anatomical direction of the cardiac extremity of the aorta, Dr. Corrigan infers, that during the contraction of the left ventricle, the heart should not strike the ribs at all, but, on the contrary, should retire more deeply into the chest. To several other theories of the heart's impulse, Dr. Corrigan next proposes objections; to these we will not advert, as it is by that under consideration at present, that the strength of his assertions can alone be satisfactorily measured. We shall, therefore, pass at once to the rationale of the impulse which he advances himself, and in order to condense as much as possible, we shall advance our own epitome of his opinions in preference to his minuter details. Rejecting all the ideas which have hitherto prevailed, he contends that the *impulse* against the side, is produced, not by the tendency to straighten the curvature of the aorta, not by the recoil of the blood impelled by the contraction of the left ventricle, not during the contraction of that ventricle, and, therefore, not synchronous with the *pulse*; but that the impulse takes place during the dilatation of the left ventricle; that the contraction of the left auricle is the impelling force that urges the left ventricle against the side during the time of its diastole while it is full of blood; and, finally, that the *impulse* is synchronous with the contraction of the auricle, and anterior to that of the ventricle, or what may be colloquially termed *the pulse*.

Now, to establish this extraordinary and novel proposition, it is evident that Dr. Corrigan must afford us proof by the most convincing experiments, that the doctrines we have so long upheld are utterly erroneous. To remove these prejudices, he must have recourse to direct and downright demonstration, and of this Dr. Corrigan appears completely aware, and he selects his proofs with great ingenuity; first, from the simple examination of the impulse and pulse in the human subject; secondly, from the assistance of the stethoscope; thirdly, from the auscultation and inspection of the heart's action in the lower animals; and lastly, from the pathological records of heart disease.—We turn to the first step in the demonstration he proposes to afford.—If his assertions be correct, if the impulse is occasioned by the contraction of the auricle, and take place during the diastole of the ventricle, it is evident that we should feel the heart's impulse at the side anterior to the pulsation in the wrist.—Can this by any possibility be the case? Dr. Corrigan emphatically answers, that

"It is."—"Is it so?" he exclaims, "or are they even synchronous? They are not. I know that my readers will be startled by the assertion, but let them before they discredit my assertion, that they are not synchronous, but that the impulse of the heart against the side is anterior to the arterial pulse, place the index finger of the right hand on the point where the heart in their own persons beats most strongly, at the same time keeping the thumb or fore-finger of the left, upon the radial artery of the right hand.—*When the heart is beating slowly and forcibly*, they will perceive distinctly, that the first tap is against the ribs, the second from the pulse."—"The second tap indicates the precise moment of the contraction of the ventricle; the first, the heart's impulse against the side; the contraction of the ventricle is therefore posterior to the impulse of the heart. An effect cannot precede its cause: therefore the contraction of the ventricles which follows, cannot produce the heart's impulse which has gone before. This experiment on the side and wrist was conclusive in my mind, that the contraction of the ventricles could not be the cause of the heart's impulse, and convinced me that the statements given by all physiologists of the heart's movements were erroneous, and that the position assumed by all, namely, that the heart strikes the side when the ventricles were contracting, was false."

Such is Dr. Corrigan's first proof; to the rest we shall next devote a cursory notice.—Stethoscopically the sounds of the heart may be divided into two, the *long* and the *short*. All authorities before Dr. Corrigan concurred in the assertion, that the long sound was occasioned by the contraction of the ventricle, and of course was synchronous with the pulse, but this Dr. Corrigan also denies, and affirms that it occurs synchronous with the impulse, but anterior to the pulse; according to him, therefore, the long sound appertains to the auricular systole, and the short is synchronous with the pulse or ventricular contraction.—The validity of this assertion must of course rest, in a great degree, on the authority and testimony of *competent* stethoscopists, and accordingly Dr. Ferguson, a lecturer on that

subject, one of the favourite pupils of Laennec, a gentleman in extensive practice, of the highest reputation, and admitted to possess a knowledge of stethoscopic evidence superior to most, and inferior to no practitioner in existence, comes forward and fully corroborates Dr. Corrigan's assertion, that the long sound and impulse are synchronous, and that both these precede the simultaneous occurrence of the short sound and the arterial pulsation.

Dr. Corrigan proceeds still further, he lays open the thorax of the living rabbit, and accompanied by Dr. Ferguson, inspects and listens to the sounds and motions of the heart, and they thus both hear and see that the long sound accompanies the auricular systole, and is synchronous with the impulse—that the short sound occurs at the same moment with the systole of the ventricle, and the pulse in the arterial branches. They next examine the motion of the exposed heart in the frog, the same phenomena take place; and they moreover notice, that it is towards the conclusion of the auricular systole that the left ventricle touches the sides, and that not by its apex, but by a part of its lateral surface.

Lastly, Dr. Corrigan cites a case from Bertin, in order to prove by pathological facts, that inordinate impulse may take place under a morbid condition of the heart, totally incompatible with the possibility of that impulse, depending on an increased *active* condition of the left ventricle. The auricle was hypertrophied, the ventricle *in a state of ramollissement* with the exception of one hypertrophied spot, yet throughout the progress of the case the most inordinate impulse was observed. It is to be remembered also, that the pericardium was adherent throughout.

Such is the skeleton statement of Dr. Corrigan's peculiar opinions, a perusal of his details will point out several corroborative circumstances, which we have not space to consider. We have, however, presented enough to show, that should competent observers coincide with his doctrines, Dr. Corrigan will have established not only an important physiological proposition, but a pathological guide to the knowledge of heart disease, of value in proportion to the despair with which the achievement of any means of sure diagnosis, was hitherto contemplated by all those who were competent to pronounce opinions on the subject. Already Dr. Corrigan has applied his principles to the elucidation of pathology; but in this attempt we shall not follow him at present, for it is worse than superfluous; nay, it is dangerous in the extreme, to add an additional story to a building, the foundation of which is still regarded as insecure; and thus, since some objections of no mean tenor have been adduced against Dr. Corrigan's hypothesis, we must consider these before we proceed any further in our task.

Of the reviews and critical notices which this pamphlet has called forth, we shall only particularize two: of the first, which appeared in the Medico-Chirurgical Review, we are compelled, in justice to Dr. Corrigan, to write in terms of no laudatory kind; this we regret, for many, for self-evident reasons; but we have ever held, and ever will maintain, that the administration of public justice should be superior to every other consideration. The reviewer throughout indulges in the most virulent invective against the object of his criticism; but the motives which instigated this malignant proceeding may be sufficiently recognised in the following extract, which we are astonished the innate hypocrisy of the writer did not induce him to suppress:—

"It seems that Dr. Corrigan was hardy enough to publish a memoir on the *bruit de soufflet* in THE LANCET, and what is yet more worthy of imitation, he is sufficiently simple and confident to refer to it. We know not what Dr. Corrigan may think, nor do we particularly care; but this we will say, that such a mode of coming before the public is not calculated to win the ears of the respectable part of the profession in England. The acquaintance of the costermongers of St. Giles's is not a good passport to society in St. James's, and so such writers as Dr. Corrigan will discover."

Such is the exordium with which the diatribe against his intended victim is commenced; we shall only say of it, that, in permitting his rancorous anger to glare through the veil of hypocrisy he should rather have drawn around him, he served the cause he intended to defeat, and secured from every unbiassed man a more attentive consideration for Dr. Corrigan's suggestions than it is probable they would have otherwise procured. Throughout the sequel of the article, though we scrutinised it with the most

good-humoured diligence, we were unable to detect a single rational objection to Dr. Corrigan's views; all was either abuse, misstatement of the author, or commentaries on his opinions, the ignorance of which was only paralleled by the extraordinary recklessness with which they were put forward. Thus we find that the reviewer flatly denies the truth of Dr. Corrigan's statement of the simple experiment with the finger on the pulse and heart:—

"Since commencing the present review (he states), we have examined a considerable number of patients in a large hospital, and the result of our observations is in decided opposition to Dr. Corrigan's. In some few cases, indeed, the impulse and first sound were *slightly anterior* to the pulse at the wrist, but even here the interval was extremely minute," &c.

Mark, in a *hospital*, and next let the reader remember the necessity of having slow-beating hearts for the experiment. Neither can we perceive why a hospital was selected; at any rate, it is scarcely the place to look for slow-beating hearts, except among the convalescents, and here, we believe, the reviewer did find the fact to be as Dr. Corrigan describes. Further, in the notice of the case from Bertin, he argues on it as one of hypertrophy of the left ventricle, suppressing the accompanying ramollissement of its sides; of course he plumes himself especially on the adhesion of the pericardium; and this, with extraordinary empiricism, he pronounces "sufficient to explain the increased impulse described in the cases." So it would, doubtless, if the ventricular fibres had been in a state competent to perform their functions at all; but unless he endows the pericardium with more contractility than it is generally believed to possess, unless he dubs it a provisional lieutenant to the left ventricle, we cannot see how it *accounts* for the moderate impulse, any more than it would, if there existed in this subject no left ventricle at all. We trust M. de Ville will procure a cast of this worthy's skull, and mark the prodigious development which his "causality" must have attained.

At page 133, we find "Dr. Corrigan asserts that impulsion is not the gauge of hypertrophied ventricle; we assert it is;" yet, in a preceding page, he observes—"Is not Dr. Corrigan aware that adhesions of the pericardium will produce a most extensive and violent action of the heart, with little or no hypertrophy at all!!! Thirdly, as to his knowledge of the sounds of the heart. He says, p. 182, "The very dull sound heard in hypertrophy of the ventricle, resembles *bruit de soufflet*." The merest tyro in the use of the stethoscope would laugh at this assertion; the dull sound accompanying hypertrophied ventricle, resembles *bruit de soufflet* about as much as the blow of a double drum resembles the whistling of air through a key-hole. Finally, Dr. Corrigan adduces cases of heart disease in support of his views, in which the *bruit de soufflet* came *before* the pulse, and in *place* of the long or first sound of the heart. The reviewer says, the *bruit* was produced by the regurgitation backwards from the ventricle into the auricle; if so, the *bruit* should have been synchronous with the pulse. It was not so, the pulse terminated the *bruit*, just as in the healthy heart it terminates the natural dull sound. The reviewer's explanation was, therefore, palpably absurd.

When we say that had this article contained a single rational argument or sensible remark, we should have availed ourselves of it; in the course of this investigation we will be the more readily believed, since we shall immediately quote at considerable length from a paper published by Dr. J. Hope, in the filthiest vehicle that ever sprung from the prostitution of typography; as the memoir itself, however, is comparatively good, and is in some degree relevant to the point at issue, we quote from it on the same principle that would induce us to pick up a diamond though it glittered on a dunghill. Before, however, we proceed to the matter of Dr. Hope's memoir, we have strongly to object to a species of bad taste, perhaps, to be sure, depending on inexperience, which, throughout the entire, is painfully manifest; we allude to the circumstance of Dr. Hope's prefixing his name to the article, yet, in its progress all through, speaking in the plural number, aping the sneering style of the illiberal article we have already noticed, and condescending to put forward personalities, which, though by usage tolerated in a reviewer, are still utterly unpardonable in authenticated strictures. The arguments this gentleman op-

poses to Dr. Corrigan we shall now advert to, in the order of, and as far as they relate to, the epitome of Dr. Corrigan's opinions, we have sketched for our readers.

In the first place, then, he totally denies the accuracy of Dr. Corrigan's hydraulic experiment, and deduces from it entirely opposite conclusions, which he applies to the corroboration of the opinions adopted by Hunter, Bostock, and Laennec.

"We have repeated (says Dr. Hope) Dr. Corrigan's experiments before a number of competent judges, and cannot arrive at the same conclusions. We employed an elastic tube *a foot long*, placed its orifice in the various positions indicated by the author, and found that, in every instance, its only tendency was to straighten itself, and throw the extremity outwards. The tube had a natural curve, which offered a resistance equivalent to two or three ounces. We placed a finger underneath the curve, and raised it nearly to the straight position; yet the injected fluid had power to raise it from our finger, and elevate it still further. Though we directed the orifice to one side, at the same time supporting the arch on the other, the straightening tendency was still equally exerted upwards, and it was observed invariably to commence at the end of the curve nearest to the syringe. We therefore quote the author's own experiment as conclusive in favour of Hunter and Bostock's position, and fatal to his own; the fallacy which deceived him we suspect to have resided in the length of the tube, the weight of which co-operating with any slight reaction generated at the discharging orifice, threw the tube accidentally into the positions which he describes. In our experiment, this source of fallacy was obviated by our employing a tube so short as to counteract by its rigidity the effects of its gravity and discharging reaction."

It requires no microscopic eye to detect the errors into which Dr. Hope has fallen here, we reserve our observations on them till we develope the main links in the chain of his disquisition. It will be remembered that independently of the error in physics which Dr. Corrigan imputes to Hunter and Bostock, that he also asserts, that even if the aorta did tend to straighten itself, yet that this effort would, in consequence of the position of the arch obliquely forward from left to right, move the heart away from the left side, and towards the right.

"This objection," says Dr. Hope, "involves an oversight. The author did not perceive that the lateral movement would be opposed by the atmospheric pressure in the lungs, and also by the central attachment of the pericardium: now, as an elastic tube will tend to elevate its loose end, whatever may be the direction of its curve with respect to its orifice, the motion will take place in that direction where it meets with least resistance, and this in the case of the aorta and heart, is forward and towards the left; Hunter's explanation, therefore, is accurate and consistent with the anatomy of the heart and great vessels."

The question of the synchronism or non-synchronism of the impulse and arterial pulsation, next engages Dr. Hope's attention.

"Willing to mistrust," he proceeds, "our own previous observations, we set soberly and dispassionately to work, to inspect the author's experiment, not only on ourselves but extensively on our friends, non-medical as well as medical, and likewise on horses—assisted by Mr. Field, a highly intelligent veterinary surgeon, and expert auscultator. It was universally agreed on, that except in cases of disease, the actions were synchronous. The few who thought that in some cases, especially in horses, they did not correspond with mathematical precision, allowed that to pronounce such a discrepancy, non-synchronism would be giving undue latitude to the imagination, in colloquial phrase, straining the point."

As it is to these two points we attach the chief importance in this inquiry, we shall not follow Dr. Hope further than to say, that he objects to Dr. Corrigan's experiment on rabbits, in consequence of the extreme rapidity of their pulsation, and to that on the frog, that he makes the twofold objection of the heart being single, and the auricle much more powerful in proportion to the ventricle, than it is in warm-blooded animals.—Dr. Hope then proceeds to offer some objections to the notions entertained by Dr. Corrigan concerning the sounds of the heart, and concludes by narrating experiments of his own, and deductions he derives from them, and which, he conceives, will set at rest all disputed points concerning the motions, sound, and rythm of the heart. To these we may advert on a future occasion, meanwhile we may observe that Dr. Hope's "novelties," though got up in a workmanlike manner, are still too imitative and consecutive of Dr. Corrigan's, to attract the same attention or demand the same investigation.

We have thus traced the outline of Dr. Corrigan's opinions, omitting the shadowing and perspective, and merely confining

ourselves to the figure of the delineation, we have proceeded in the same manner with Dr. Hope, and shall now briefly contrast the two sketches as they stand. In the first place, we must notice the defects we have observed in Dr. Hope's performance, with reference to his remarks on Dr. Corrigan's experiment on the tubes, it is evident that Dr. Hope is not entitled to argue from his repetition of the experiment, for this simple reason, that it was not Dr. Corrigan's experiment he performed at all. In our experiment, he says, we avoided Dr. Corrigan's fallacy, by employing a tube so short as to *counteract by its rigidity* the effects of its gravity, and *discharging reaction*. Surely, Dr. Hope, you don't call this a repetition of Dr. Corrigan's experiment; if you do, your mistake is in the inverse proportion to the length of your tube. Again, Dr. Hope says, "the fallacy which deceived Dr. Corrigan, we suspect to have resided in the length of his tube, the weight of which, co-operating with any slight reaction, generated at the discharging orifice *accidentally*, threw the tube into the positions which he describes."—Now all this is little short of silly verbiage. Dr. Hope, we perceive, has yet to learn precision of language and mathematical perception; the word "*accidentally*" is peculiarly unfortunate. Did the tube assume the positions represented by Dr. Corrigan, or did it not? if it did, it was in consequence of the action of certain hydraulic laws, not accident, which you should have investigated; and these hydraulic laws you as evidently contravened by the substitution of a rigid tube, insusceptible of the influence of their operation. Again, of the two experiments, which represented the heart and the aorta, curvature, descent, and branches, with the closest resemblance? Dr. Corrigan's long tube, or Dr. Hope's rigid tube. We believe the longer the tube the more perfect will the analogy be admitted to have been. On these grounds, we give the preference to Dr. Corrigan's experiment, although it must be admitted in qualification of the whole, that both the experimentalists applied the discharging instrument at the wrong end.

In Dr. Hope's second objection he is rather more happy; namely, where he observes that the central attachment of the pericardium and the atmospheric pressure, would tend to prevent the heart retiring into the chest, even if the ventricle's contraction urged it in that direction. But ultimately this objection, though entirely valid, is of no importance to the issue of the case, because Dr. Corrigan ascribes the impulse to the contraction of the auricle, which of course would act on these attachments, and be re-acted on by them, in precisely the same manner.

In the next place, we find that the question of synchronism or non-synchronism of pulse or impulse is left, as far as facts or arguments are concerned, very nearly in the same position in which Dr. Hope took it up. With the best possible dispositions for making them as synchronous as identity itself, yet he is compelled to "strain a point," and admit a slight degree of synchronism in some cases, especially in horses, animals in whom, according to Dr. Corrigan's views, from the slow motions of their hearts, this non-synchronism was to be anticipated, if it existed at all. The coincidence will be here observed of the admission by both reviewers of the non-synchronism in a *slight* degree, which is quite as much as we could expect them to admit, and their candour in doing so at all, does them no little credit.

With respect to Dr. Hope's objections to the results deduced from Dr. Corrigan's experiments on the rabbits, we feel the following conviction,—that Dr. Hope, in the first place, is not borne out in his palpable insinuation, that the motions were so quick, that Dr. Corrigan's imagination outstript his observation, and that he described from fancy what the rapidity of the cardiac motions should have rendered it impossible to inspect with accuracy. Dr. Corrigan fell in to no such blunder; he tells us emphatically at the commencement, the necessity for slow-beating hearts, and he explains to us precisely the circumstances under which the examination of the rabbit was conducted; meanwhile Dr. Hope, wishing to avoid his opponent's supposed error, falls into the directly opposite fault; and in repeating his experiments (the only repetition we can perceive is, that he used the same species of animal), he allows the motions of the heart to become so slow and abnormal, that no rational conclusion can possibly be deduced from their inordinate successions.

In the next place, with respect to Dr.

Hope's stethoscopic observations, we set them at little value, when compared with the great weight which must be attached to Dr. Ferguson's evidence, for reasons already mentioned. We trust Dr. Hope will pardon us for this rude parallel, but we speak our conviction, which we shall be happy to alter if he can prove it to be erroneous. It should not be lost sight of, that Dr. Hope does not make even the most distant allusion in his memoir, to the extraordinary case cited by Dr. Corrigan from Bertin.

In conclusion, our own opinions on the average merits of the question are briefly these: *According to the evidence* adduced, we conceive Dr. Corrigan has made out a good and firm case; but we think also, that much further testimony is required, before his researches can be legitimately turned towards the improvement of diagnostic science; experiments must be performed with care, manual and stethoscopic examinations in the healthy state must be assiduously repeated: finally, pathological records must be kept with a wary eye, on the cases accompanied by *auricular* disease. When this body of evidence shall have been procured, then and then only can Dr. Corrigan's ideas be universally adopted. This much he has done at least, that the experimentalists must be able indeed, who can clearly set aside the statements he has advanced. On Dr. Corrigan and his coadjutors we shall pass no eulogium at present, further than to remark, that if by perseverance they succeed in establishing the beautiful and novel doctrines he has originated, physiology and pathology will rank him and them among the most eminent of their contributors. Before we close the article we must further say, that in the only *practical* investigation we had leisure to institute, namely, the manual examination of pulse and impulse in the human subject, we invariably found the nonsynchronism Dr. Corrigan describes. Finally, it is by no means the least important corroboration of his views, that on the 16th of March, without any knowledge whatever of Dr. C.'s opinions, Dr. Pigeaux, a young French physician of the brightest promise, read a memoir before the Academie Royale de Medecine, in Paris, in which identically the same ideas were proposed, with the notice of Pigeaux's paper, extracted from the Journal Hebdomadaire of April: but we shall close our observations.

"Academie Royale de Medicine, Seance de 16 Mars,—Nouvelle Analyse des mouvemens du cœur, M. Pigeaux lit une note fort importante sur les mouvemens du cœur et les bruits qui les accompagnent. Suivant ce jeune et habile observateur, 1. Les bruits entendus pendant les contractions du cœur resultent du frottement du sang contre les parois internes de cet organe. 2. C'est pendant la diastole des ventricules, que le cœur vient frapper de sa pointe la region precordiale, en sorte que ce mouvement n'est pas synchrone a celui du pouls. 3. Le bruit clair rapporté aux oreillettes appartient aux ventricules, et le bruit sourd correspond aux contractions auriculaires, et non a celles des ventricules. 4. Il n'existe pas, comme l'a dit M. Laennec, de veritable repos entre la systole et la diastole."

We translate this important notice in order that every individual may mark the strong corroboration which its coincidence affords to Dr. Corrigan's opinions.

"Royal Academy of Medicine, sitting of the 16th of March,—New analysis of the motions of the heart. M. Pigeaux read a very important notice on the motions of the heart and the sounds by which they are accompanied. According to this young and able observer, 1. The sounds heard during the contractions of the heart result from the friction of the blood against the internal surface of that organ. 2. It is during the *diastole* of the ventricles that the heart strikes with its apex against the precordial region, so that this motion *is not synchronous* with the pulse. 3. The clear sound attributed to the auricles belongs to the ventricles, and the dull sound corresponds to the auricular contraction, and not to that of the ventricular. 4. No actual period of repose intervenes between the systole and diastole, as M. Laennec has asserted."

CORONER FOR MIDDLESEX.

To the Editor of THE LANCET.

SIR,—Few can contemplate the zeal, ability, and independence, which have distinguished your proceedings in an official capacity, without wishing you, in the cause in which you have offered yourself as a candi-

date, success equal to that which marked your exertions in the editorial department.

That a coroner ought to be conversant with the studies of law and medicine, is now a truth pretty well established; and that only a modicum of legal knowledge is absolutely necessary for such a functionary, is a fact as strongly ratified. It is not my motive to draw comparisons between the two professions, any more than they are connected with the present argument; but admitting the requisitions to be equal,—that the portion of legal, ought to be equivalent to that of medical, information;—I do maintain that the acquaintance with the identical sciences, and those collateral with medicine and surgery, ought to be deemed paramount to every other qualification, as being in itself of primary import. It has been said, that "as a practical profession, the study of the law requires but a moderate portion of abilities;" and few at the present day will be inclined to dispute this doctrine, if a certain crown lawyer be regarded as the "maximum" of forensic attainment, while, at the same time, the numerous recorded failures of those members of our own profession, who have arrogated to themselves claims to superiority over their "subordinate brethren," would afford sufficient proofs that superior talents are necessary to constitute a perfect surgeon. There is not a volume of the journal of which you are editor that will not attest your fitness; as far as the above comprehend the pre-requisites for the office for which you as a candidate contend.

That you, Sir, who in his own province so triumphantly defeated him who, from certain recent arbitrary and oppressive transactions, ought to be entitled (ex-officio) the Rhadamanthus, rather than "the guardian of the laws," should be crowned with triumph in the present competition, is the humble and sincere wish of

ZENO.

FIRING FOR LAMENESS IN HORSES.

To the Editor of THE LANCET.

SIR,—Having observed in THE LANCET for the 4th of September, at page 901, a letter from Mr. George Fenwick in reply to a paper of mine inserted in THE LANCET for August the 21st, on the operation of firing for lamenesses, I am induced to offer the following answers to Mr. Fenwick's questions, though I cannot help prefacing them with a short comment on the asperity with which Mr. Fenwick has expressed his objections to my method of operating. I can conceive only two circumstances under which I might have deserved such a reply, viz., if I had been a quack-doctor, or had rashly promulgated the formidable mode of operating, merely upon shallow theoretical grounds, but the truth is, I have been constantly practising these *cautery lesions* (or to use Mr. Fenwick's awful words, this "*horrid and monstrous operation*") for the last *fifteen* years and upwards, both in the army and in private practice, and there is no veterinary instrument that I take in hand with more confidence than the firing-iron, after a cautious investigation of the seat of lameness.

But to the point, Mr. Fenwick says, that in order to put the case beyond all cavil, he will suppose me called on to fire a diseased fetlock-joint, and asks, would I *then* make the *red-hot iron* pass boldly through the skin by crucial incisions till it reached the cellular tissue immediately covering the ligaments, tendons, periosteum, &c.; and further inquires how I can avoid cauterizing and dividing the important nerves, arteries, and veins, that are immediately under the skin: had he not limited me to a particular joint, I would have selected the hock-joint for the illustration, and attempted to have shown the *radical cure for bone-spavin*. No; he would fain hope that the *fetlock* might prove a *fast-lock* to the new system altogether.

As I have never practised the operation of firing by either mode unless palpable disease has existed, it is requisite that I should briefly describe the nature of one or two diseases common to the fetlock-joint of the horse, say, of the fore-leg.

I will suppose a tumour on the inner or the outer ancle contiguous to either of the sesamoid bones, this is a very frequent cause of lameness, the pulley-like joint formed by the sesamoid bones being more or less involved; this enlargement or bulge is to be met with rather more frequently on the *inside* of the fetlock-joint very near to the *cutting place*, but distinguishable from the effects of striking, in an instant, by the experienced eye; it is a diffused swelling consisting of a condensation of cellular tissue and lymph effused under the skin, while the integuments preserve their original or natural thinness, but there is usually an accompaniment which completely stamps the character of the enlargement, viz., a slight projection or thickening of the inner or outer branch of the *suspensory ligament*, as may happen to be the affected side, just below the bifurcation; the other parts of the leg may be sound and clean, and the patient tolerably free from lameness whilst at rest, but when put to ordinary work lameness ensues immediately. I need scarcely add that this consolidation of parts originates in nine cases out of ten in a sprain of the elastic suspensory ligament. Sometimes *both* sides of the joint are similarly affected. Now for the *cure*, and I promise Mr. Fenwick I will stick to my *text*.

Enlargement of the Fetlock Joint on the inside of the Fore Leg.

Lesion the 1st.—A longitudinal incision with the *red-hot iron*, down the centre of the tumour, as if intending to make a section of it; this single line comprises about half the cure, and therefore it must be boldly done; merely penetrating the skin from top to bottom will not suffice, unless by its retraction a considerable furrow is immediately produced of a certain size, generally about the width of a large straw; to effect this, it may be necessary also to penetrate a part of the *cellular tissue*, but Mr. Fenwick, in his alarm for the safety of the important nerves, arteries, and veins, must have forgotten, that in the *morbid* condition of the joint, those structures are completely shielded by the adventitious substances immediately under the common integuments.

Lesion the 2nd.—A longitudinal incision with the same instrument down the posterior part of the fetlock-joint, in a line with the back tendon or flexor, completely through the skin, but without any additional effort to dilate the furrow; the penetration of the skin only at this part will instantly expose the cellular membrane, forming the immediate covering to the sheath of the tendon.

Lesion the 3rd.—A corresponding incision down the front of the joint, with the same caution, to penetrate the skin only. The *transverse* incisions between the three longitudinal will complete the operation; these must be performed with a differently-shaped instrument, much smaller; but yet a red-hot iron, which should also *penetrate* the skin, and will require great dexterity on the part of the operator, as it is expedient the lesion of the skin should be effected with the least possible dilatation of the line. On this part of the undertaking depends the proportion of *blemish*, but when dexterously performed, it contributes much to the *neatness* of the fired leg.

I have, however, on some occasions, dispensed with this division of the operation, and depended solely on the three or four longitudinal incisions, carried to the utmost depth that my experience would sanction, which have proved successful.

My text reminds me that I have yet to expose the periosteum. A bony excrescence in front of the fetlock-joint, rather inclined to the inside, though sometimes to the outer, is another very frequent cause of lameness, and from its inconsiderable size, is often an obscure cause of lameness, varying in size and shape from a pea to a horse-bean, but frequently of the dimensions of a walnut and upwards; this exostosis is generally situated near the superior and anterior edge of the large pastern bone, where it articulates with the large metacarpal bone to form the fetlock-joint. In such a case I depend principally on the single longitudinal incision laying bare the periosteum part of the large pastern bone.

Of course *splints* may be effectually treated in like manner.

With respect to additional cruelty attending this mode of firing, I admit that it is a natural inference to draw; but I doubt the fact, for this reason,—less tumefaction follows my method than attends the other *next best plan of firing*, which consists in passing the instrument *all but through the skin* at the time of the operation, so that the remaining portion ulcerates through in the course of the suppurative process, by about the third or fourth day. Now when we reflect that there is no organ more abundantly supplied with nerves than the skin of the horse, it must be obvious that we diminish, instead of increase, the sufferings of our patient by the *perfect*, rather than the *partial* lesion of such organization,—much more might be urged, but as space does not permit, it must suffice to state a well-known fact, that Mr. Thomas Turner, of Croydon, and myself, perform this formidable operation with our patients *standing* on their legs in the proportion of about nine cases out of ten, I mean without casting the horse.

In the next place Mr. Fenwick calls upon me, and I confess with good reason, to state in what period my patients *recover*, to which I answer, I require them to remain at my own stables for about a fortnight from the day they are operated on, they are then fit to be turned to grass, straw yard, or a large open shed, without requiring any farther medical treatment whatever, where they remain for the space of two months at least, before they are taken up for work, and of course, if longer rest is allowed by the owner, so much the better for the case, but two months will suffice.

When it has happened that the owner has been unwilling to promise that period of rest, nothing has ever induced me to risk my credit by operating in a single instance.

Instead of commenting on the concluding sentence of Mr. Fenwick's rather uncerimonious epistle, in which he states to your readers, that I do not appear fully to comprehend the subject. I shall be content with quoting his own avowal as a specimen of *profound knowledge* in these matters, after his 30 years' practice in this great metropolis.—Vide the 2nd Column of Mr. Fenwick's Letter.

"For the few instances that have come under my observation, when the animal may happen to have made a sudden and violent struggle before the iron could be withdrawn, and it has *unfortunately* gone *through* the skin and burnt the cellular tissue, but without injuring either *tendon*, *ligament*, *or periosteum*, I have invariably found in-

flammation and *sloughing* follow, and many months have even elapsed before the poor animal has been again fit for service; and in one instance I recollect the *animal became nearly useless from the consequences.*"

In conclusion, Mr. Editor, I must avail myself of this opportunity of stating, that I beg to decline any further controversy, as my leisure is at present occupied in preparing for the press a work on this operation, and the various diseases for which it is applicable.

I am Sir,
Your obliged, humble servant,
JAMES TURNER.

Horse Infirmary, 311, Regent Street,
Sept. 11th.

UNIVERSITY OF LONDON.

LETTER FROM PROFESSOR PATTISON.

To the Editor of THE LANCET.

SIR,—On my return from Paris, where I had been on a visit for the last six weeks, my attention was called to Number 362 of THE LANCET, in which I observe you have printed, on the suggestion of a Mr. Alexander Thomson, a pamphlet which was published ten years ago by Dr. Chapman of the University of Pennsylvania, with the view of injuring the rival institution with which I was then connected—the University of Maryland.

When my reputation was slandered in the United States, I felt called on, because I was there a stranger, to enter into a full and circumstantial vindication of it. But when the same calumnies are circulated in my own country, where I am well known, I feel that the same necessity does not exist. The principal charge contained in the "Correspondence," &c. was fully inquired into by my countrymen more than twelve years ago, and the estimation in which my character had been held previously, was in no degree affected by it. The facts of it were also most carefully investigated by the council of the University of London, when I was a candidate for a chair in that institution, and the result was, I was elected to the distinguished office I now occupy. Neither pains nor expense were spared by Dr. Chapman to give currency to this calumny in the United States; above 10,000 copies of the "Correspondence," &c. were circulated; there was not a post town to which a copy was not sent. But as the circulation of my "Refutation," &c. was co-extensive, the result was, that all the attempts made to injure my reputation proved abortive, and I can proudly assert that no private gentleman ever visited that country, who was more respected than myself, or who, when he left it, was held in higher estimation by the first and most eminent of its citizens. In evidence of this, I send you a few extracts from some of the numerous testimonials I received after a residence of six years, when I became a candidate for the Professorship of Anatomy in the University of London.

Certificate by the Honourable the Heads of Departments of the Government of the United States of America.

Washington, Nov. 5th, 1825.

Having been requested to state our impression of the reputation and standing of Mr. Granville S. Pattison, the Professor of Anatomy in the University of Maryland, we take pleasure in saying, that his reputation as a teacher is of the highest order, and, according to our information, has contributed eminently to the present prosperity and high standing of that institution: and with regard to moral habits, he is, so far as we are informed, altogether above exception. It is only of the reputation of Professor Pattison that we are able to speak; but from the high character which he sustains, both as a lecturer and a gentleman, we should regret as a public loss any change of pursuit which should remove him from our country.

JAMES BARBOUR, Sec. at War.
SAM. L. SOUTHARD, Sec. of the Navy.
WM. WORT, Attorney-General.
RICHARD RUSH, Sec. of the Treasury.
H. CLAY, Secretary of State.

Letter from the Right Rev. BISHOP KEMP, *of Maryland.*

Baltimore, Oct. 31st, 1825.

Sir,—Although I should regret the loss of Mr. Granville S. Pattison, as a distinguished and able Professor in our University, my esteem for him and my regard for truth induce me to state what I know and believe with regard to his talents and merits. He has filled the chair of Anatomy in the medical department of the University of Maryland for four years, during which time it has risen to great celebrity. His minute knowledge of the human body, with all its connexions and sympathies; the precision of his statements, and the clearness of his illustrations, place him in the first rank of lecturers. Besides, the estimation in which he is held in society will render his loss severely felt and sincerely lamented.

I am, Sir,
With much regard, Yours, &c.
JAMES KEMP,
Bishop of the P. E. Church in Maryland, and Provost of the Univ. of Maryland.

Extract of a Letter from his Excellency CHRISTOPHER HUGHES, *Minister Plenipotentiary from the U. S. to the Court of Sweden, addressed to* THOMAS CAMPBELL, Esq.

"Professor Pattison having explained his views as respects an appointment in the University of London to many of his friends here, we wish him success, but we know how serious a loss to us and to our country must be the consequence of that success. His great professional talents are appreciated here; his high professional honour, and the virtues of his private life, have endeared him to the first and the most eminent of my countrymen," &c.

Extract of a Letter addressed to ALEXANDER BARING, M.P., *by* ROBERT GILMAN, Esq., *of the City of Baltimore, in which reference is made to* Dr. CHAPMAN'S *calumnies.*

"Amidst the difficulties and jealousies which a stranger has necessarily to encounter with rival claimants for professional honours, this gentleman (Mr. Pattison) has won the friendship and respect of the society of this city, and his departure from us will be sincerely regretted by myself and many of my friends."

Extract of a Letter addressed to the Council by his Excellency ALBERT GALLATIN, *late Minister Plenipotentiary to the Court of H. B. M.*

"I have been personally acquainted with Mr. Granville S. Pattison in Baltimore, where he was highly and universally respected. He has filled the chair of anatomy in the University of Maryland for several years in a distinguished manner, such indeed as has raised the character of the medical department of that institution. His resignation has been a subject of regret to those who had the pleasure of his acquaintance, and is considered by all as a public loss.

ALBERT GALLATIN.
Upper Seymour Street, April 28, 1827."

Extract of a Letter from JOHN M'TARISH, Esq., *H.B.M. Consul General in the U. S.*

"As a member of society Dr. Pattison is universally respected and esteemed, by every class of that community where he has for five years resided, and where he has proved himself a most active and useful member," &c.

I might add many more extracts to the same effect, but these I consider quite sufficient to show the estimation in which my character was held in America, and how completely Dr. Chapman had failed in his attempts to injure it.

Being convinced from experience that every man in a public situation must expect to have his conduct occasionally misrepresented, I had determined not to notice numerous attacks reflecting on me as a Professor in the University of London, which have of late found a place in your journal; and which, it seems, have in a great measure emanated from the young gentleman above alluded to. Mr. Alexander Thomson, it appears, from his own confession, has been the active agent in exciting the students to prefer complaints to the council; and although he has never been a student of my class, he has taken the trouble to address a memorial to the council, praying for my dismissal from the University.

The council of the University have most carefully investigated all the charges which have been brought against me, as a teacher of anatomy. So far from having discouraged them, or offered "bribes" to get the students to withdraw them, they have even requested the individual pupils who signed the memorial, to bring forward in writing every complaint which they could urge against my competency; and after the most patient examination of them, they have decided that they are utterly without foundation. That the estimation in which my talents and zeal as a professor are held, has suffered nothing in the minds of the distinguished gentlemen who compose the council, by this investigation, will be sufficiently proved by the statement of the fact, that on the resignation of Mr. Bell, of the Chair of Surgery, they have done me the high honour to offer it for my acceptance, in addition to the one I before occupied.

Having furnished this communication, I have only, in conclusion, to state, that assured of the confidence of the Heads of the University, it is my determination in future to devote the whole energies of my mind to the performance of the important duties which they have entrusted to me, as their Professor of Anatomy and Surgery, and to treat with silence any charges which my enemies may think fit to bring forward against me.

I have the honour to be, Sir,

Your obedient servant,

GRANVILLE S. PATTISON.

33, St. James's Place, St. James's,
Sep. 13th, 1830.

SEDATIVE QUALITIES OF THE WEB OF THE BLACK SPIDER.—The web of the black spider has received commendation from many respectable sources, as a sedative agent, capable of calming with peculiar ease and certainty, morbid excitability of the cerebral and nervous systems. On the credit of those qualities it has been employed in the various forms of temulence (exhaustion from intoxication), and not without a share of reputed success, sufficient to entitle it to consideration in that state of constitutional irritation. In the summer of 1827 we tried this article in many cases, and in full doses. To test its qualities, it was given, where the state of the patient admitted, uncombined with opiates. When thus used, its effects were generally partial or doubtful, and its powers inadequate to the production of tranquillity or sleep. In one case only have I found this substance to exert great or decided sedative attributes. This was the case of an intelligent young man, (in private practice,) who, after consuming, by his own report, three quarts of brandy, in thirty-six hours, fell into a state of temulent excitation so excessive, that he was incapable of keeping a recumbent, or even a sitting posture, for more than a moment, but paced his chamber with a ceaseless step for two days and nights. He was not delirious, on the contrary his conversation was rational, though hurried and vehement. But he was so far under the influence of spectral hallucination, that if he closed his eyes for a moment, day or night, he was instantly visited by a host of phantoms of frightful aspect; hence chiefly his aversion to lie down, or make any voluntary effort to sleep. This patient took opium, opium with camphor, and black drop at short intervals, and in full doses, until the quantum of opiate approached the utmost limit of probable safe administration, without even partial relief of constitutional irritation, or any apparent proneness to sleep. The temulent excitement kept unabated for twenty-four hours, the second night passed in constant vigilance, locomotion, and mental excitement; and it seemed probable that excitation so intense, protracted, and unremitting, must soon lapse into delirium or convulsions. At this time, the morning of the third day, (the second of my attendance,) he began the use of the fresh web in pills of five grains every hour. Its effect was prompt and unequivocal. He calmed, even sensibly to himself, with every dose, and watched with desire for the time of repeating the pills. The first effect of the web was to abate his restless movements about the room, he became disposed to sit down, and kept his chair, with short intervals of walking, for some hours. In the evening he consented to go to bed, got up once or twice, but returned to bed without difficulty, took an opiate at night, the first for eighteen hours, and slept continuously for eight hours. The cure was completed without difficulty, by repeating the web less frequently next day, with quiet, suitable nourishment, and another opiate at night. The patient spoke emphatically, both the first and second days, of the soothing influence produced by the pills. He was not at the time informed of their composition.—*Dr. Wright. Amer. Jour.*

[*From a Correspondent.*]

Mr. PORTER, who gave evidence in favour of Long, is not, as represented, surgeon-general to H.M. forces, his rank being merely local; he never received a shilling of his Majesty's money, consequently is not entitled to the distinction of Surgeon-general to the Army in Jamaica.

MR. LAWRENCE'S LECTURES.

IN conformity with our promise given some time back, we this week publish the last of Mr. Lawrence's lectures; and Vol. II. of THE LANCET for the year 1829-30 is now concluded. From the multiplicity of pressing matters in which we have been engaged during the last fortnight, we find it to be utterly impossible to adhere to our usual practice, namely, that of giving the Index with the last Number of the Volume. It is, however, in a forward state of preparation, and it will accompany the second or third Number of the succeeding volume.

TO CORRESPONDENTS.

A VAST number of communications have been received, many of which will be inserted at future periods. Those letters which require particular notice, shall be referred to next week.

ERRATA.

In Mr. Dixon's case of Dropsy in our last Number, page 898, column 2, line 38, for "*acet. tart.*" read *antimon. tart.*; and in line 39 of the same column, for 3ss read 3ss.

END OF VOL. II., 1829-30.

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ERRATA in Mr. Lawrence's lectures, last volume; page 722; line 34; for uniformity of the, read, *unity or*.—P. 725; col. 2; l. 46; for precipitatum, read, *nitrico-oxidi*; l. 49, for nitratum, read, *nitratis*.—P. 762; c. 2; l. 35; for hunge, read, *hunger-cur*.—P. 765; c. 2; l. 63; for ætas, read, *dies*.—P. 775; l. 31; for bone, read, *periosteum*.—P. 811; c. 2; l. 38; for straight, read, *carved*.—P. 812; c. 2; l. 43; for saturated, read, *strong*.—P. 814; c. 2; l. 53; for tartarizatum, read, *tartar-izati*; l. 54; for tartar, read, *tartarized*.—P. 823; c. 2; l. 11; for would, read, *would not*.—P. 804; l. 4; for patient, read, *part*: l. 12; for philosophers, read, *pathologists*.—P. 865; l. 28; for innocent, read, *instances*.—P. 878; c. 2; lines 2, 11; for squamies, read, *squamæ*. In the present volume;—P. 1; l. 4; for psoriasis, read, *pura*.—P. 625; c. 2; l. 42; for line above, read, *linea alba*.

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